TREATISE ON BASIC PHILOSOPHY

Volume 7

EPISTEMOLOGY AND METHODOLOGY III: PHILOSOPHY OF SCIENCE AND TECHNOLOGY

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MARIO BUNGE

Treatise on Basic Philosophy

VOLUME 7

Epistemology & Methodology III:

PHILOSOPHY OF SCIENCE AND TECHNOLOGY

PART I

FORMAL AND PHYSICAL SCIENCES

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This volume is part of a comprehensive *Treatise on Basic Philosophy*. The treatise encompasses what the author takes to be the nucleus of contemporary philosophy, namely semantics (theories of meaning and truth), epistemology (theories of knowledge), metaphysics (general theories of the world), and ethics (theories of value and right action).

Social philosophy, political philosophy, legal philosophy, the philosophy of education, aesthetics, the philosophy of religion and other branches of philosophy have been excluded from the above *quadrivium* either because they have been absorbed by the sciences of man or because they may be regarded as applications of both fundamental philosophy and logic. Nor has logic been included in the *Treatise* although it is as much a part of philosophy as it is of mathematics. The reason for this exclusion is that logic has become a subject so technical that only mathematicians can hope to make original contributions to it. We have just borrowed whatever logic we use.

The philosophy expounded in the *Treatise* is systematic and, to some extent, also exact and scientific. That is, the philosophical theories formulated in these volumes are (*a*) formulated in certain exact (mathematical) languages and (*b*) hoped to be consistent with contemporary science.

Now a word of apology for attempting to build a system of basic philosophy. As we are supposed to live in the age of analysis, it may well be wondered whether there is any room left, except in the cemeteries of ideas, for philosophical syntheses. The author's opinion is that analysis, though necessary, is insufficient – except of course for destruction. The ultimate goal of theoretical research, be it in philosophy, science, or mathematics, is the construction of systems, i.e. theories. Moreover these theories should be articulated into systems rather than being disjoint, let alone mutually at odds.

Once we have got a system we may proceed to taking it apart. First the tree, then the sawdust. And having attained the sawdust stage we should move on to the next, namely the building of further systems. And this for three reasons: because the world itself is systemic, because no idea can become fully clear unless it is embedded in some system or other, and because sawdust philosophy is rather boring.

The author dedicates this work to his philosophy teacher

Kanenas T. Pota

in gratitude for his advice: "Do your own thing. Your reward will be doing it, your punishment having done it".

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PREFACE TO
PHILOSOPHY OF SCIENCE AND TECHNOLOGY

This is a systematic study in the philosophy of science and technology, or PS & T for short. It struggles with some of the so-called Big Questions in and about contemporary S & T, i.e. questions supposed to be general, deep, hard, and still *sub judice*. Here is a random sample of such problematics. Is verbal psychotherapy scientific? Is political economy ideologically neutral? Are computers creative? What is the ontological status of machines? Is engineering just an application of basic science? What is language? Are there laws of history? Which are the driving forces of history? Which is the most fruitful approach to the study of mind? Are genes omnipotent? Are species collections or concrete systems? Do the earth sciences have laws of their own? Is chemistry nothing but a chapter of physics? Does contemporary cosmology confirm theology? Has the quantum theory refuted scientific realism? Is there a viable philosophy of mathematics? How are we to choose among alternative logics? What is the ontological status of concepts?

These and other questions of interest to philosophy, as well as to science or technology, are tackled in this book from a viewpoint that is somewhat different from the dominant PS & T. An instant history of our discipline should help place our viewpoint. Modern PS & T began together with modern science and it was cultivated by scientists and philosophers until it became professionalized in the 1920s. At this time it took a *logical* turn: it was equated with the logical analysis and orderly reconstruction of scientific theories. Experimental and field work were deemed to be ancillary to theorizing, and technology was praised or deprecated, but hardly analyzed. Later on PS & T took a *linguistic* turn: only the languages of S & T seemed to matter. Facts, problems, theories, experiments, methods, designs and plans were overlooked. More recently, PS & T took a *historical* turn: everything was seen from a historical viewpoint. The logic, semantics, epistemology, ontology and ethics of S & T were declared subservient to its history or even irrelevant. Even more recently there have been attempts to force PS & T to take a *sociological* turn. Facts are said to be the creation of researchers, who would act only in response to social stimuli or inhibitors; there would be neither norms nor objective truth.

I believe the time has come for PS & T to take, or rather retake, a *philosophical* turn: to investigate the logical and semantical, epistemological and ontological, axiological and ethical problems raised by contemporary S & T, leaving the sociological and historical studies to social scientists. The time has also come to approach the problematics of PS & T in a *scientific* fashion, by paying close attention to current developments in S & T and checking philosophical hypotheses against the findings of S & T. At least this is the approach adopted in the present volume.

Although this book is part of an eight-volume treatise, it is self-contained: it can be read independently of the others. Moreover, each chapter can be read independently of the others. The book is addressed to philosophers, scientists, technologists, and culture watchers. It may be used as a textbook in a one year advanced course in PS & T. Each chapter may also be used in a course in the corresponding branch of PS & T.

To facilitate its use as a textbook, the present volume has been divided into two parts. Part I is devoted to the philosophy of the formal and physical sciences, whereas Part II covers the philosophy of the biological and social sciences as well as of the technologies.

ACKNOWLEDGEMENTS

I owe much to the many students who took my courses in PS & T: they asked interesting questions, shot down half-baked ideas, and provided valuable information. I am no less indebted to hundreds of specialists with whom I have had the privilege of discussing a host of problems in the course of four decades of scientific and philosophical research. These interactions have helped me identify and work out some of the methodological and philosophical problems that working scientists and technologists confront or skirt. They have also provided both stimulation and control.

I am particularly indebted to: my teacher Guido Beck (physics), Dave Bernardi (information technology), David Blitz (social work), Stephen Brush (history of science), George Bugliarello (engineering), Carlos F. Bunge (physics), Marta C. Bunge (mathematics), María E. Burgos (physics), Mike Dillinger (linguistics and psychology), Bernard Dubrovsky (physiology and psychiatry), Antonio Fernández-Rañada (physics), Emilio Flor-Pérez (geology), Máximo García-Sucre (chemistry), Enrique Gaviola (S & T policy), Jacobo M. Goldschvartz (physics), Ted Harrison (astronomy), Jacques Herman (sociology), Luis Herrera (astronomy), Andrés J. Kálnay (physics), Bernulf Kanitscheider (philosophy), Bernardo Kliksberg (management science), Hiroshi Kurosaki (philosophy), José Leite-Lópes (physics), Jean-Marc Lévy-Leblond (physics), Ralph W. Lewis (biology), Jean-Pierre Marquis (philosophy), Storrs McCall (philosophy), Mauricio Milchberg (information technology), Francisco Miró-Quesada (philosophy), Jesús Mosterín (logic), Jorge Niosi (economic sociology), Phineas Finn O'Jonceys (retrieving), José L. Pardos (international relations), Michel Paty (physics), Raúl Prebisch (economics), Miguel A. Quintanilla (philosophy), Osvaldo A. Reig (biology), A.C. Riccardi (paleontology), the late Jorge A. Sábato (S & T policy), Nicolás Sánchez-Albornoz (history), Yasuo Sasaki (Toyota Motor Co.), Daniel Seni (city planning), William R. Shea (history of science), Abner Shimony (physics), John Maynard Smith (biology), José Félix Tobar (engineering), Clifford Truesdell (applied mathematics), Raimo Tuomela (philosophy), Hao Wang (mathematics), Paul Weingartner (philosophy), and René Zayan (ethology). Had I listened to all their criticisms and suggestions, this would have been a better and thicker book.

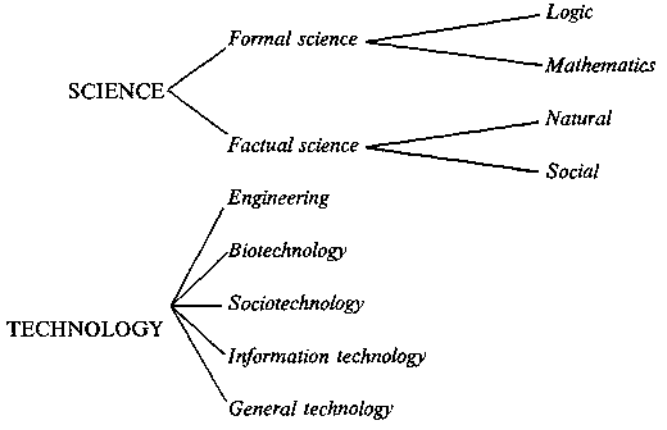
INTRODUCTION

The aims of this Introduction are to characterize the philosophy of science and technology, henceforth PS & T, to locate it on the map of learning, and to propose criteria for evaluating work in this field.

1. THE CHASM BETWEEN S & T AND THE HUMANITIES

It has become commonplace to note that contemporary culture is split into two unrelated fields: science and the rest, to deplore this split – and to do nothing about it. There is some truth in the two cultures thesis, and even greater truth in the statement that there are literally thousands of fields of knowledge, each of them cultivated by specialists who are in most cases indifferent to what happens in the other fields. But it is equally true that all fields of knowledge are united, though in some cases by weak links, forming the system of human knowledge. Because of these links, what advances, remains stagnant, or declines, is the entire system of S & T.

Throughout this book we shall distinguish the main fields of scientific and technological knowledge while at the same time noting the links that unite them. And we shall focus our attention on the following broad fields of inquiry:



All these fields share logic and mathematics (or at least a substantial part of them) and the scientific method – which, though not used in technological design, is employed in the modeling and testing of artifacts. In addition factual science, whether natural or social, shares with technology a number of ontological, epistemological and ethical principles, such as "The world is lawful", "Hypotheses may refer to unobservables but must be checked through observation", and "Empirical information must be found out not made up".

Yet in spite of this philosophical core common to science and technology, and in spite of the fact that research in these fields continues to pose new philosophical problems, most philosophers have kept far removed from them: see Table 0.1 And only three great philosophers in the entire history

TABLE 0.1. Some influentional philosophers and their interest in S & T.

	Math.	Nat. sci.	Social sci.	Tech.
Plato	×		×	
Aristotle	×	×	×	×
Bacon				×
Descartes	×	×		
Spinoza	×	×	×	
Leibniz	×	×	×	
Locke			×	
Hobbes		×	×	
Berkeley	×	×		
Hume			×	
Kant	×	×		
Voltaire			×	
Diderot				×
Hegel			×	
Bolzano	×			
Marx	×	×	×	×
Frege	×			
Peirce	×	×		
Bergson		×		
Russell	×	×	×	×
Whitehead	×	×		
Dewey			×	
Husserl	×			
Heidegger				×
Wittgenstein	×			
Popper	×	×	×	

of mankind have paid close attention to mathematics, natural science, social science, and technology: Aristotle, Karl Marx, and Bertrand Russell. (That mathematics was not precisely a strong point of Aristotle's or of Marx's, and that Russell wrote little of lasting interest about natural science, is beside the point. We are here concerned with the interest that influential philosophers have shown in the most advanced fields of inquiry.)

Worse, the subset of influential philosophers who, in addition to having shown some interest in mathematics, science, or technology, have made any true and important statement in or about either field of inquiry, is surprisingly small. (The reader is invited to draw up his own list, and is urged not to take a nationalistic stand.) The chasm between philosophy on the one hand, and mathematics, science and technology on the other, is still huge. This makes the attempt to bridge it all the more challenging.

2. BRIDGING THE CHASM

The chasm between the humanities (in particular philosophy) and the sciences (and mathematics and technology) can be bridged, nay it is already being bridged every day in three different and mutually complementary ways. One is by rendering the humanities more scientific, as when linguistics teams up with neuroscience, psychology, sociology and history. Another is by making scientists and technologists more aware of the philosophical presuppositions and implications of their own research. A third is by engaging in the cultivation of any of the disciplines dealing with S & T.

Every science of S & T has its own concept of S & T. The psychology of S & T – a branch of cognitive psychology – treats inquiry as a mental process in an individual brain, and it may ask questions such as: What neuronal systems engage in exploratory behavior or in calculation?, or What prompted so and so to pose such and such problem? The sociology (and the economics and the politology) of S & T construes S & T as a process in a community of inquirers or, better, as a dynamic subsystem of society at large. It asks e.g. what the cultural or political stimuli and inhibitors of research are, how basic research is being applied, how best to encourage innovation, or to allocate scarce resources to individuals or teams. Finally, the historian of knowledge is supposed to treat S & T as both individual (cognitive) and social processes. He may also treat S & T as a body of knowledge, feigning that it exists separately from the brains-in-society that create, diffuse, or utilize it.

In addition to these various bridges between the humanities and S & T

we have PS & T. It seems obvious that, although this discipline belongs to the humanities, it ought to make ample use of the findings of the various sciences of S & T. Unless it does, the philosopher of S & T will persist in the error of the traditional (purely internalist) philosophers and historians of knowledge, who studied knowledge as belonging to Plato's realm of ideas (or its modern equivalent, Popper's "world 3"). By proceeding in this manner the philosopher of S & T will remain estranged from the realities of S & T. He will produce descriptions of, or prescriptions for, S & T that are unrealistic and, as a consequence, will widen the chasm between philosophy and S & T instead of bridging it.

The philosopher of S & T should try and get hold of the real S & T, that which occurs in contemporary individual brains embedded in the society of today. By so doing he may do more than contribute to bridging the gap between the two cultures. He may also (*a*) enrich philosophy with a number of ideas suggested by S & T and (*b*) help S & T hone concepts and analyze propositions, spot new problems and suggest novel ways of approaching them, note links between distant fields and evaluate research projects or technological designs. By so doing he may become useful enough to S & T to earn his keep in any scientific or technological institution.

Most scientists and technologists are likely to smile, or even wince, at the idea that philosophers may tell them how to do their job. All too often they have read caricatures of scientific research or technological design. They know that real life scientists and technologists do not go about in a neat logical way but muddle through hunches and accidents, such as errors and faulty equipment. True, but most philosophers of S & T have not presumed to describe S & T but, rather, to analyze it, or reconstruct it, or discover rules for the successful conduct of inquiry. Their mistakes in these regards are no worse than some of the stories scientists occasionally tell – e.g. that the universe emerged out of nothing about 20 billion years ago, that atoms do not exist as long as they are not observed, or that the aim of science is to make careful observations.

No doubt, the good scientist (or technologist) knows how to do science (or technology) without having the logician, the methodologist, the epistemologist, or the ontologist breathe down his neck – as long as he does not come across a philosophical problem. But such an accident is bound to happen during an epistemic crisis or a revolution. And at such a juncture a philosopher, if conversant with the problem in question, may help provide analysis, criticism, or even constructive suggestion. In any case we may expect more from the cooperation of the philosopher with the scientist or

the technologist than from their competition, let alone from their mutual contempt or disregard.

Philosophies of S & T are usually classed into descriptive and prescriptive. (And the former is often accused of being at best identical with history, at worst incorrect, whereas the prescriptive or normative philosophies are felt to be insolent.) However, this dichotomy is incorrect, for there are four other tasks that a philosophy of S & T may perform: analysis, criticism, rational reconstruction, and synthesis. Analysis and criticism, which are also involved in scientific or technological research, are obvious. What is not obvious is that S & T needs a dose of philosophical analysis and criticism – e.g., of their presuppositions, basic concepts or hypotheses, and procedures. As for rational reconstruction, or systematic analysis, it is the orderly reconstruction of theories and methods. Finally, philosophical synthesis deals with the general principles (ontological, epistemological, etc.) underlying the practice of S & T. In short, the various aspects of PS & T are description (of actual practice), prescription (of the best practice), analysis, criticism, rational reconstruction, and synthesis. A job for a lifetime, even if confined to a single field of inquiry.

3. TOWARDS A USEFUL PS & T

It is well known that most scientists and technologists do not listen to living philosophers. Both sides are to be blamed for this. The former for rarely reading anything outside of their own specialties, for often distrusting generalities, and for neglecting the study of the foundations of their own fields. And philosophers are to be blamed for being, as a rule, out of touch with scientific and technological advances, and for employing a jargon unintelligible to scientists or technologists.

We must try to change these attitudes that perpetuate the chasm between philosophy and S & T. The change can come only from training scientists and technologists to take a keener interest in generalities, and from reforming philosophy so that it may become attractive to researchers in other fields. In order for a philosophy of S & T to be of interest and of use to scientists and technologists, it must

(*a*) adopt the scientific approach;

(*b*) deal with real S & T, not the highly idealized and often ridiculous picture taken from popularizations;

(*c*) investigate philosophical problems that arise in the course of current

scientific or technological research, or in thinking philosophically about it;

(*d*) propose precise solutions to such problems – if necessary solutions in the form of exact (mathematical) theories;

(*e*) criticize wrong approaches to research or design;

(*f*) stimulate research or design by proposing new problems, hypotheses or methods, rather than inhibit the exploration of new ideas;

(*g*) distinguish genuine from bogus S & T, deep from shallow ideas, moral from immoral policies;

(*h*) be aware of the social matrix and the history of S & T.

In short, we should aim for a PS & T that is relevant and topical, exact and systematic, critical and fertile, as well as in close touch with the other sciences of S & T.

To better indicate the kind of PS & T capable of advancing knowledge, in particular a knowledge of S & T, let us draw up a short and nearly random list of some of the problems it should investigate. This will also show that the new PS & T, far from being a chapter of epistemology, extends throughout the territory of philosophy and even somewhat beyond it.

1. *Logical problems*. What are the formal relations between two given concepts, hypotheses, or theories? What changes can we expect in a theory if the underlying logic is altered? Is it true that logic is an empirical science and may therefore be expected to change radically in the light of new scientific developments?

2. *Semantical problems*. How is factual interpretation assigned to constructs? What are the referents of the theory of evolution: species, populations, or ecosystems? How can the concept of partial truth be exactified?

3. *Epistemological problems*. What is the relation between a factual proposition and its referents? How are metastatements to be evaluated? Is the use of subjective probability ever justified in S & T?

4. *Methodological problems*. What is an indicator? What are the conceptual (in particular philosophical) presuppositions of measurement and experiment? How can the degree of confirmation of a theory be measured?

5. *Ontological problems*. How do formal objects exist? What is a law? What are artifacts?

6. *Value-theoretic problems*. What are the roles of valuation and preference in S & T? How are cognitive, practical and moral values related? Is it possible to recast decision theory in terms of objective values and probabilities?

7. *Ethical problems*. Is science morally neutral? Do technologists have moral responsibilities? Is it possible to base ethics on S & T?

8. *Esthetic problems.* Do scientists and technologists encounter any esthetic problems? Are beauty and usefulness mutually compatible? When do we say that a theory is beautiful or a proof elegant?

9. *Socio-philosophical problems.* How do ideologies affect the development of S & T? What are the philosophical underpinnings of a given S & T policy? Can philosophers do anything to change the way the public "perceives" S & T?

10. *Historico-philosophical problems.* Was it by chance that logic emerged in the first political democracy in history? Why was the study of nature reviled before the Renaissance, and exalted thereafter, in both cases *ad majorem Dei gloriam*? Why are Hume and Kant, neither of whom could read Newton's *Principia*, still widely regarded as the philosophers of the new science?

The philosopher of S & T who works on any of these or related problems, and abides by the principles laid down earlier in this Section, may do more than earn a salary and have fun: she can also be an active participant in the development of S & T rather than a distant and passive onlooker. In fact she can make contributions, to both S & T and the humanities, by:

(*a*) *digging up the philosophical presuppositions* of ideas and practices in S & T;

(*b*) *elucidating and systematizing key philosophical concepts* occurring in S & T, such as those of chance, mind, social system, meaning, truth, and explanation;

(*c*) *helping solve some scientifico-philosophical problems*, such as those of the nature of matter, life, mind, society, culture, and language;

(*d*) *proposing rational reconstructions* of research projects, scientific theories, or plans of action;

(*e*) *helping design S & T policies and plans.*

Any such work may not only advance philosophy but also S & T and even serve as a model for work in the less advanced branches of philosophy.

4. CONCLUDING REMARKS

Every expert is rightly held morally and socially responsible for the advice he offers, because his advice is part of the conceptual basis of decisions that may be beneficial or harmful to his client or to the public. The moral and social responsibility of philosophers is heaviest because their clientèle is the whole intellectual class. In fact philosophers help, for better or for worse, shape the intellectual climate of their time.

In particular philosophers of S & T, if listened to, contribute to forming attitudes – of hope or fear, love or hatred, receptivity or rejection – towards S & T. Therefore it behooves philosophers of S & T to start by becoming acquainted with the very object of their thinking, thus living up to the motto *Primum scire, deinde philosophari.* Otherwise they may promote obscurantism instead of illuminism: they may spread the idea that astronomy is no better than astrology, or evolutionary biology no truer than any creation myth; that the mind cannot be studied scientifically, or that social science is impossible; that technology is inherently evil (or good), and faith healing no worse than medicine. They may, in short, preach that "anything goes", which is indistinguishable from radical skepticism ("nothing goes").

The choice of a philosophy of S & T is a matter of great social importance in our time, when S & T lies at the very center of culture. If adopted by the people in charge of cultural policies, a wrong philosophy can maim or even kill S & T. For example, an idealist S & T policy maker would underrate laboratory and field work, as much as a positivist would overrate it. In either case the necessary balance between reason and experience would be the victim of a wrong PS & T.

Let us then try to build a PS & T both faithful to its object and capable of promoting its advancement.

FORMAL SCIENCE
FROM LOGIC TO MATHEMATICS

We start with the philosophy of logic and mathematics for several reasons. First, nowadays all of the sciences and technologies employ mathematics, which has logic built into it. Second, mathematics presupposes no knowledge of fact: it is logically, though not epistemologically, a priori. Third, for the same reason mathematics is semantically neutral: it does not describe the real world. Fourth, and consequently, mathematics is methodologically a priori: it involves no empirical procedures. In fact, the content and validity of mathematics are independent of any empirical techniques, such as finger counting and computer programming, that help carry out some calculations or construct certain proofs.

The a priori character of mathematics does not entail that it exists by itself in Plato's realm of ideas, or that mathematicians have nothing to gain from occasional contacts with science and technology. All we are stating is that, considered as a conceptual system, mathematics has no factual content and makes no essential use of empirical procedures. However, we are anticipating some results that have yet to be established. In order to obtain them it will prove convenient to begin by defining the concept of a research field. Our characterization of mathematics will result from a certain specification of this general concept.

1.1. *Two Main Types of Research Field*

Throughout this volume we shall be concerned with research fields, such as mathematics and medicine, rather than belief systems, such as religion and astrology. Since the concept of a research field will recur often in this book, we shall rehearse the definition proposed in Vol. 6, Ch. 14, Sect. 1.2:

A *family of research fields* is a set every member $\mathscr{R}$ of which is representable by a 10-tuple

$$\mathscr{R} = \langle C, S, D, G, F, B, P, K, A, M \rangle,$$

where, at any given moment,

(i) C, the *research community* of $\mathscr{R}$, is a social system composed of persons who have received a specialized training, hold strong information links among themselves, and initiate or continue a tradition of inquiry;

(ii) S is the *society* (complete with its culture, economy, and polity) that

hosts C and encourages or at least tolerates the activities of the components of C;

(iii) D, the *domain or universe of discourse* of $\mathscr{R}$, is the collection of objects of study of $\mathscr{R}$;

(iv) G, the *general outlook* or *philosophical background* of $\mathscr{R}$, is composed of ontological theses (concerning the nature of the D's), epistemological principles (about the nature of inquiry into the D's), and ethical rules (about the proper behavior of the inquirers in C);

(v) F, the *formal background* of $\mathscr{R}$, is the collection of logical and mathematical theories that are or can be used by members of C, in studying the D's;

(vi) B, the *specific background* of $\mathscr{R}$, is the collection of items of knowledge obtained in other fields of inquiry and utilizable by the C's in studying D's;

(vii) P, the *problematics* of $\mathscr{R}$, is the collection of problems (actual or potential) that can be investigated by members of C;

(viii) K, the *fund of knowledge* of $\mathscr{R}$, is the collection of items of knowledge utilized by C and obtained by it at previous times;

(ix) A is the set of *aims* or *goals* of the members of C with regard to their study of D's;

(x) M, the *methodics* (usually misnamed 'methodology') of $\mathscr{R}$, is the collection of methods utilizable by members of C in their study of D's;

(xi) there is at least one other (*contiguous*) research field $\mathscr{R}'$ in the same family of fields of inquiry, such that

(*a*) $\mathscr{R}$ *and* $\mathscr{R}'$ share some items in their general outlooks, formal backgrounds, specific backgrounds, funds of knowledge, aims, and methodics;

(*b*) either the domain of one of the two fields, $\mathscr{R}$ and $\mathscr{R}'$, is included in that of the other, or each member of the domain of one of the fields is a component of a system in the domain of the other;

(xii) the membership of every one of the last eight components of $\mathscr{R}$ *changes*, however slowly, *as a result of inquiry* in the same field or in related fields.

We call a research field *formal* if and only if all the members of its domain or universe of discourse D – clause (iii) in our definition – are conceptual, and *factual* if at least some of them are concrete or material. In other words, the formal/factual dichotomy is generated by the nature of the referents or objects of research fields. Thus, if a research field is about predicates in general, then it is formal, because "predicate in general" is a concept not a thing or a property of a thing. If on the other hand some of the predicates

in a research field (or rather in its fund of knowledge K) are interpreted in factual terms, as is the case with "younger than", then the research field is factual even when it possesses a substantial formal background F. All of the factual sciences, pure or applied, natural or social, and all of the technologies with the exception of information technology, are factual research fields. Formal science is the only research field where all "fact" is fiction.

The radical difference between constructs and concrete things may best be seen in a few examples. *Example 1* There is no circumstance under which the identity "2 = 1 + 1" could fail to hold for numbers. But it may fail for things if '+' is interpreted as physical union: for example the union of two liquid droplets may result in a single droplet. *Example 2* Reference frames are representable by coordinate systems but are not identical to them. The former are things, the latter are concepts, and the former can move relative to one another, whereas the latter cannot. *Example 3* Nothingness, or the null thing, has no physical properties. However, nothing has prevented us from defining the *concept* $\square$ of null thing, which has no real counterpart, as that which adds nothing to a real thing (Vol. 3, Ch. 1, Sect. 1.1). This is not an idle fiction, for it allows us to state the general law that nothing comes out of, or ends up in, nothingness.

The formal/factual dichotomy stems from Leibniz's classical partition of the set of all propositions into *propositions de raison* and *propositions de fait* (Leibniz 1703 Bk. IV, Ch. 2, Sect. 14, and Ch. 11, Sect. 13). Such distinction is multiple: ontological, semantical, and methodological (Bunge 1984c). It is ontological because the objects satisfying a truth of reason, such as a tautology or a geometrical theorem, are constructs (e.g. propositions or conceptual triangles). On the other all the objects that satisfy a truth of fact are concrete (real, material) things. The difference is semantical because, among other reasons, an abstract formula, if satisfiable, is so in one or more models. (For example, the associative law is satisfied by the multiplication of numbers). In contrast, a factual statement, if true (to some extent), is true only *of* the only real world there is, not *in* a model (or conceptual "world").

As for the methodological differences between propositions of reason and propositions of fact, they boil down to these: (*a*) some of the former, namely the conventions (such as definitions), need not be justified except for their practical convenience, whereas all factual statements are in need of (empirical) justification: (*b*) the formal statements in need of justification are justified either by proof (deduction) or by checking satisfaction (or compliance with assumptions and definitions). And both operations are strictly

conceptual. On the other hand every factual assumption calls for, at some point or other, some empirical operation – preparation, observation, measurement, or experiment – in addition to conceptual operations.

True, some mathematical propositions *seem* to be also truths of fact. For example, the theorem that the sum of the internal angles of a plane triangle equals 180° seems to hold for drawing triangles because it can be confirmed by measurements on them. This objection to the formal/factual dichotomy misses the semantical and methodological differences we have just noted. In fact, the theorem in geometry refers to a conceptual triangle, whereas the theorem in physical geometry refers to a material triangle, or rather a material thing of (*approximately*) triangular shape. And measurement *confirms*, to an excellent approximation, the theorem about physical triangles without *proving* it for conceptual triangles. No proposition is at the same time a *vérité de raison* and a *vérité de fait*. Hence both objective idealism (e.g. Platonism) and empiricism (as well as naive materialism) are wrong. More in Sect. 5.2.

We assume that pure mathematics, as classically exemplified by arithmetic, geometry, and analysis, is a formal research field, i.e. one such that all of its objects or referents are constructs, and all of its truth claims must be substantiated by purely conceptual means. This is the *conceptualist* thesis of our philosophy of mathematics, sketched in Vols. 1 and 2, and to be worked out in subsequent sections. This thesis may be traced back not just to Leibniz (and Bolzano) but also to Plato. Indeed, Plato was the first to recognize the ideal (or conceptual) nature of mathematical objects, as well as the purely conceptual character of mathematical procedures. (See Wedberg 1955.) However, we hasten to warn that our conceptualist thesis is metaphysically different from Plato's doctrine of forms or pure ideas. Far from holding that ideas exist by themselves in a realm of their own, we hold ideas to be brain processes, and constructs to be equivalence classes of such (Vol. 4, Ch. 4, Sect. 4.3., and Vol. 5, Ch. 1, Sect. 2.1.) Ours is then a *methodological* not a metaphysical dualism.

The formal/factual dichotomy, and the conceptualist thesis on the nature of mathematics, are not universally accepted by philosophers – in fact they are becoming unpopular among them. They are rejected by the radical empiricists and pragmatists, who hold that all propositions derive ultimately from experience, and all truth claims must ultimately be justified empirically (e.g. Kálmar 1967). Nor have they been admitted by such naive realists as the early Wittgenstein and the dialectical materialists, who hold that every true proposition "mirrors" or "reflects" some feature of the world. And our

dichotomies have been ignored by the conventionalists, to whom all truths are stipulations or disguised definitions.

Some of the best known recent attacks on our dichotomies are those of Quine, Putnam, and Lakatos. Quine (1970) – and also Wang (1974) – rejects them in the name of the unity of "the whole interlocked scientific system". (Rejoinder: such unity is real and indispensable, but it is not harmed by the formal/factual distinction. Distinguishing does not entail detaching. The working partnership of the formal and the factual disciplines is indicated in our schema by the remark that every science has a formal background, so that it contains some mathematical propositions and methods.) Putnam (1975) claims that atomic physics has refuted ordinary logic and employs a logic of its own, where distributivity fails – which would dispose of the thesis that logic is a priori. (Rejoinder: The view that atomic physics has refuted ordinary logic derives partly from a wrong corpuscular interpretation of electron diffraction experiments: see Ch. 2, Sect. 5.2. Besides, quantum theory has ordinary mathematics built into it, and in turn the logic underlying ordinary mathematics is ordinary logic. More on this in Sect. 2.2.) Finally, Lakatos – in the wake of Gonseth, Pólya, Kálmar, and others – holds that, except for proofs, mathematical research does not differ radically from research in other fields, as it, too, draws inspiration from experience and proceeds by trial and error as well as by conjecture and counterexample. (Rejoinder: The basic methodological unity of all the sciences and, indeed, of all rational discourse and activity, is not in question and it does not prove that there are no profound semantical and methodological differences between mathematics and factual science).

We shall return to the philosophy of mathematics in several places, particularly in Sect. 5.2. Let us now see how our conceptualist thesis affects the reading of our definition of a research field. To begin with, the first two components of the ten-tuple $\mathscr{R}$ are hardly touched by our thesis on the a priori nature of mathematics, except to remind us of the ambiguity of the term. In fact 'mathematics' may be understood as denoting a *social group* (the mathematical community), the *activity* of members of such group (i.e. mathematical research), or the *product* of such activity (i.e. the changing body of mathematical knowledge). There is no harm in admitting all three acceptations of the word as long as they are not mixed up. In fact it is convenient to use them all, in order to forestall *sociologism*, which focuses on mathematics as a social group (e.g. Wilder 1981, Restivo 1983); *pragmatism*, which stresses activity at the expense of what the activity consists in and accomplishes (e.g. Wittgenstein 1978); and *idealism*, which detaches

products from producers and consumers (e.g. Plato, *Republic* vi.510 C-E).

However, it may be necessary to state explicitly what the first two components of the 10-tuple $\mathscr{R}$ point to, even though it should be obvious. They suggest that mathematical research is done by members of a mathematical community hosted by a sufficiently advanced society. The community is held together by a tradition and by information flows. There is no such thing as the mathematical loner totally cut off from both the tradition and the community. And mathematical research (as distinct from the invention of a few crude and stray mathematical ideas) did not start before the dawn of civilization, and it does not prosper in contemporary backward societies. Indeed, the level of advancement of mathematics in a society is an important indicator of the level of refinement of its culture. (This is a rebuttal of Pascal's opposition between *esprit de géométrie* and *esprit de finesse*.)

The conceptualist thesis does call for a nontrivial specification of the remaining coordinates of the 10-tuple $\mathscr{R}$. To begin with, the *domain D* (or universe of discourse, or reference class) of mathematics is, according to our thesis, the collection of constructs. To be sure mathematics can be successfully applied to studying the real world. Moreover such study has often motivated the invention of mathematical ideas. Nevertheless it remains true that pure mathematics deals only with *êtres de raison*. When such constructs are employed in the study of reality, they are assigned an extra (factual) interpretation, as when a value of a distance function is interpreted as the separation between two physical objects.

Clause (iv) in our definition concerns the *general outlook* or *philosophical background* of a research field. Whatever the explicit philosophy (if any) that a mathematician may espouse, he works on a *tacit philosophy* including the following tenets. Firstly, he treats the objects of his research (functions, spaces, or what have you) as *sui generis* objects devoid of physical properties. In fact he proceeds *as if* they were self-existent in a world of their own, to the point that he prefers to say that he discovers or finds his results rather than claiming that he has constructed or invented them. Secondly, his work is theoretical: he deals with constructs, not concrete things; consequently he makes neither measurements nor experiments. Thirdly, he abides – just like the factual scientist – by a moral code that commands not to make up anything arbitrarily (except perhaps definitions), not to plagiarize, and not to keep the results of his work from others.

The *formal background F* of mathematics – clause (v) in our definition – is, trivially, mathematics itself. But of course every branch of mathematics

has, at a given point in its historical development, a formal background that is only a part of the totality. For example, the propositional calculus has an empty formal background, i.e. it does not presuppose anything; on the other hand set theory presupposes the theory of deduction.

The *specific background B* of mathematics – the 6th component of $\mathscr{R}$ – is empty, i.e. mathematics need not borrow any items from other research fields. On the other hand workers in every other research field are forced to borrow bits and pieces of mathematics, which they use as tools.

The *problematics P* of mathematics – clause (vii) of our definition – is of course the collection of all possible problems, concerning constructs, that can be handled with the methods in *M* (the tenth coordinate of $\mathscr{R}$), and involving, at some point or other, purely conceptual (in particular theoretical) proofs or disproofs. Shorter: a mathematical problem is a problem the solution to which involves formal (i.e. nonempirical) proofs or disproofs.

The *fund of knowledge K* of mathematics – the 8th component of $\mathscr{R}$ – is the collection of results obtained previously by mathematicians and which are still valid. As is well known, the rate of decay or attrition of mathematical knowledge is the slowest. True, modern mathematicians have been able to improve on the work of Apollonius, Eudoxus, Archimedes, Euclid, and other ancient mathematicians. Still, that work stands on the whole; this cannot be said of ancient physics, with the sole exception of fragments of statics, hydrostatics, and ray optics.

The *aim A* of mathematics, or rather mathematicians – the 9th component of $\mathscr{R}$ – is of course to pose and solve mathematical problems. (The most ambitious of all such problems is the construction and inter-relation of theories.) Since mathematical problems do not concern the world – although some of them appear in the study or the control of the real world – pure mathematicians do not try to understand, let alone steer, nature or society. Only applied mathematicians, who are really factual scientists or technologists in disguise, have this other aim.

The *methodics M* of mathematics – item (x) in our ten-tuple – is composed, like that of factual science, of the general method of science plus an expanding collection of special methods or techniques. The former boils down to the sequence: *Formulate the problem-Invent hypotheses or theories-Test the solution.* The last step, testing, may consist in constructing a proof, finding a counter-example, or merely checking whether the candidate (e.g. the presumed theorem) satisfies the conditions of the problem. All such operations are exclusively conceptual, even though they may require the assistance of instruments other than the brain, such as pencils and com-

puters. As for the special techniques, sufficice it to recall those of triangulaton and integration for the measurement of areas, and the integration of a function by expanding it into an infinite series and integrating term by term.

The *continuity* condition (xi) is satisfied both locally, i.e. by every branch of mathematics, and globally, by mathematics as a whole. In fact there are no isolated chapters of mathematics: the latter makes up a conceptual supersystem. Consequently every mathematical specialist can talk to colleagues in at least one other field of mathematics. Moreover, mathematics interacts vigorously with factual science and technology: it lends them formal tools, and receives in exchange new problems and new tools – namely calculators and computers. Mathematics is thus at the very center of the science and technology system: though conceptually self-contained, it is not self-sufficient, let alone selfish. This is why those who speak the language of mathematics can talk to specialists in all the advanced sciences and technologies. Only philosophers can compete with mathematicians in universality – particularly if they understand the universal language.

Finally, the *changeability* condition (xii) is met automatically as long as there are active research mathematicians, because the job of every investigator is to innovate in his field, not just to teach it or use it. Thus it is believed that ancient mathematics died when Hypatia of Alexandria was stoned by a christian mob. There was almost no mathematical creation in Medieval Western Europe: there was only the occasional study of a few Greek mathematical texts, usually misunderstood. Original research was not resumed vigorously until the Renaissance: for more than one millennium mathematics had shrunk and degenerated from a research field into a belief system. The same may happen again if the current suspicion of pure science prevails.

So much for our general characterization of mathematics. Let us now study in some detail some of the peculiar traits of mathematics, i.e. those that mark it off from other research fields.

1.2. *Some Peculiarities of Mathematics*

Mathematics can be studied from within or from without. The internal study of mathematics can proceed on two levels: it can bear on mathematical objects, such as sets or algebraic systems, or on propositions about such objects. In the former case one conjectures or proves mathematical statements, in the latter one deals with metastatements, i.e. statements about statements. For example, the formulas "$p \vee p \Leftrightarrow p$" and "$p \,\&\, p \Leftrightarrow p$" are

mathematical (more precisely logical) propositions, whereas the statement that they are duals of each other is a metastatement, and so it belongs in metalogic. Likewise, when one tries to prove the consistency of a given abstract theory by exhibiting a model (example) of it, one engages in metamathematical research. The same holds for studies of completeness, categoricity, decidability, and the like.

We shall make only passing remarks on metamathematics, which is part of mathematics – just as a philosophical reflection upon philosophy is itself part of philosophy. Metamathematics is secure in the hands of mathematicians, who have advanced it tremendously in the course of its short history. (See e.g. Kleene 1952, Rasiowa and Sikorski 1970, Hilbert and Bernays 1968, 1970, Barwise ed. 1977.) We shall be far more interested in the philosophy of mathematics, which is one of the various external studies of mathematics – others being the psychology, sociology, and history of mathematics. Unlike metamathematics, which is hardly one century old and yet fairly advanced, the philosophy of mathematics, though at least 25 centuries old, is still underdeveloped. This is how two mathematicians interested in the subject have recently evaluated it: the philosophy of mathematics, is "dormant since about 1931" (MacLane 1981), it "has not progressed beyond 1931" (Smorynski 1983) – 1931 being the year Gödel published his incompleteness theorems.

The philosophy of mathematics is mostly a collection of interesting open problems and of ill-grounded opinions. Moreover, it is fragmentary rather than systematic. What is worse, it is quite isolated from the other branches of philosophy; in particular, none of the well known philosophies of mathematics is a component of a philosophical system consonant with contemporary mathematics and factual science. There is not even consensus on the problematics of the philosophy of mathematics. Thus a well-known reader (Hintikka 1969) sidesteps all the classical questions on the nature of mathematics, to concentrate on technical problems in the logical foundations of mathematics. In sum, the philosophy of mathematics is in poor shape. Therefore it poses one of the most interesting and pressing challenges to any philosopher who is aware that the most advanced and exact of all sciences has the most backward, woolly and dogmatic of all philosophies.

In this section we shall grapple with a few basic problems in the philosophy and psychology of mathematics, namely: What do mathematicians typically do?, How do they proceed: what kind of methods do they use?, What is the universe of discourse (reference class) of mathematics?, What

is the meaning of mathematical constructs?, What is mathematical truth (in contrast to factual truth)?, What is the difference between pure and applied mathematics?, and Do mathematicians discover or invent?

What do mathematicians do? They are supposed to think hard about conceptual problems of certain kinds. More precisely, mathematicians carry out computations (e.g. integrations) or invent new algorithms (e.g. schemes of numerical computation); they hypothesize or prove theorems in existing theories; they introduce new concepts, theories, or even entire branches of mathematics; they organize (e.g. axiomatize) known bodies of mathematics; they simplify algorithms, proofs, or theories; they analyze (in particular criticize) their own work or that of others; they investigate global properties of theories (e.g. completeness); they help colleagues in science or technology solve some of the mathematical problems that occur in them – and much else. All such tasks are strictly conceptual.

The nature of such tasks determines the way to go about them. Fingers, pencils, rulers, and computers can help, but they cannot do mathematical work because they are brainless. For example, a computer cannot work in number theory because it works with numerals not numbers, figures not geometrical objects, symbols not what these stand for. (For the difference between constructs and the signs or symbols that represent them see Vol. 1, Ch. 1.)

It has become fashionable to claim that mathematics is not fundamentally different from factual science because mathematicians often proceed by trial and error and they may employ material tools such as compasses and calculators. (E.g. Lakatos 1976, Davis and Hersh 1981.) Actually all this shows is that mathematicians are not disembodied spirits but whole organisms whose brains include sensorimotor systems connected with the rest of the body. It also shows that a mathematician uses a single brain whether to prove a theorem or solve some practical problem. Like anyone else, the mathematician is bound to use analogy and induction, and to try conjectures until hitting on the correct solution. (See Pólya 1954 for a beautiful array of examples of nondeductive reasoning in mathematics.)

If we wish to find out what sets mathematics off from other research fields we must look for differences not for similarities. An extremely simple example will suffice to show where the difference lies. Suppose we wish to prove that, for any quadruple of real numbers a, b, A, and B, $aA = bB$ if, and only if, $a/b = B/A$. An ordinary mathematician solves this problem by recalling the definition of number division in terms of number multiplication, namely: "For any real numbers x, y, and z, $x/y = z =_{df} x = yz$". An em-

piricist (and also a vulgar materialist) philosopher might propose, instead, studying the behavior of levers of arm lengths a and b, and respective weights A and B, in equilibrium states. By measuring these quantities he would in fact find that his results satisfy the given theorem, which is structurally identical with Archimedes' law for balanced levers. But of course he has only *exemplified* the theorem instead of proving it. (And even exemplifying the theorem in a physical case will require rounding off the measurement results so as to mask the errors.) He has not proved that the theorem holds for infinitely many combinations of real numbers a, b, A, and B. (See Perdang 1983 for an optical "proof" of Brouwer's fixed point theorem.)

In short, mathematicians are human beings and, like all humans, they employ certain thought patterns common to all human beings; they are also capable of drawing inspiration from activities in other walks of life. But they train themselves to think of constructs in general terms. To a mathematician an example may suggest looking for a pattern or refuting it. An accumulation of examples (i.e. what would constitute substantial evidence in factual science) is no substitute for a proof. Moreover, just as in science, individual cases only pose the problem of finding the underlying patterns characteristic of a system of constructs. Mark the words *system* and *pattern*, because they are the clues to our next question: What is mathematics about?

Historians of mathematics have noted that, until about mid-nineteenth century, the bulk of mathematical research was concerned with individual constructs, such as particular figures, equations, functions, or algorithms. From then on, and particularly since mid-20th century, mathematics has been conceived as the study of *conceptual systems*, such as groups of transformations (or even the whole category of groups in general), families of functions (or even entire functional spaces), and topological spaces (such as metric spaces in general). (Caution: Bourbaki, Bernays and others call 'structure' what others, e.g. Hartnett 1963, call 'system'. We stick to our convention in Vols. 3 and 4 that every structure is the structure *of* some object: that it is the set of all the relations among its components – the internal structure – plus those among the latter and the environment or context of the system, which can be empty – the external structure.)

Every mathematical system ("structure") can be characterized in either of two ways: (*a*) as a set equipped with a structure consisting of one or more operations or functions defined on that set (e.g. Bourbaki 1970); (*b*) as a collection of objects together with one or more morphisms relating those objects – i.e. a category (e.g. MacLane 1971). (Actually the second concept

subsumes and supersedes the first.) We distinguish two kinds of mathematical system: (*a*) *concepts*, such as that of the Euclidean plane, or that of the family of all the circles on that plane, and (*b*) *theories* (hypothetico-deductive systems) about conceptual systems in sense (*a*) – e.g. plane Euclidean geometry.

Example 1 The *system of natural numbers*, as specified in part by Peano's five axions, is $\mathcal{N} = \langle \mathbb{N}, 0, ' \rangle$, where $\mathbb{N}$ designates the set of non-negative integers, 0 a distinguished element of $\mathbb{N}$, and $'$ the successor relation. The latter generates all of the members of $\mathbb{N}$ out of 0 – e.g. $2 = 0''$. The conceptual system $\mathcal{N}$ is the subject or referent of another conceptual system, namely elementary number theory. *Example 2* A *semigroup* is a system of individuals glued together by a binary associative concatenation operation $*$: $\mathcal{S} = \langle S, * \rangle$, where S, the base set of $\mathcal{S}$, is an arbitrary (abstract) set. The entire set S may happen to be generated by concatenating a single individual a with itself a number of times, i.e. by forming $a*a$, $a*a*a$, etc. *Example 3* A *theory* is a system $\mathcal{T} = \langle P, \vdash \rangle$, where P is a set of propositions including logic, and $\vdash$ the relation of (syntactic) entailment, or logical consequence. In our systems-theoretic terminology, P is the *composition*, and $\vdash$ the *structure* of $\mathcal{T}$. A theory is *formal* if all the referents of the members of P are conceptual, and *factual* if some of them are concrete (material, real). If $\mathcal{T}$ is factual, then P contains a set of *semantic assumptions* ("correspondence rules") that specify the factual content of $\mathcal{T}$. These semantic assumptions are of the forms "*c refers to f*" or "*c represents f*", where *c* is a construct occurring in some *P*'s, and *f* is a factual item, such as a material thing, a property (or state) of it, or an event (change) in it. There are no such assumptions in pure mathematics. On the other hand a theory in applied mathematics is characterized by a non-empty set of semantic assumptions.

Talk about semantics is essentially talk about meaning or truth. What is the meaning of mathematical constructs? In our semantics (Vols. 1 and 2) only predicates, such as "$\in$", and propositions, such as "$1 = 0$", have meaning. Hence sets are devoid of meaning even when their members are meaningful. But of course sets allow one to construct objects endowed with meaning, such as the predicate which, to each ordered pair $\langle x, y \rangle$, with $x \in \mathbb{R}$ and $y \in [-1, 1]$, assigns the proposition "$\sin x = y$". Meaning, then, can emerge by combining meaningless concepts, or it can be submerged by collecting meaningful concepts into sets. E.g., numbers are meaningless because they are neither predicates nor propositions.

But what is meaning? This question is particularly important in view of the fact that some complaints against standard mathematics boil down to

the accusation that it is full of "meaningless" concepts and even propositions. (See e.g. Bishop 1967, Stolzenberg 1970.) Unfortunately those critics do not tell us what they mean by "meaning". In our semantics the meaning of a construct in a given context equals its sense together with its reference. (Vol. 2, Ch. 7.) In turn, the sense of a predicate or formula in a given context (e.g. theory) equals the set of all the predicates (or formulas respectively) that are logically related to it, i.e. that entail it or follow from it. As for reference, let us recall that we construe a predicate as a function pairing individuals, or n-tuples of such, to the propositions containing the predicate. Those individuals are precisely the referents of the predicate. More exactly, the reference class of a predicate is the union of all the sets occurring in its domain. (For example, the reference class of "give" is the union of donors, acceptors, and things given.) And the reference class of a proposition is the union of the reference classes of the predicates occurring in the proposition. (For example, the reference class of "All mathematicians love some theories" is the union of the class of mathematicians and that of theories.) In sum, the *meaning* of a predicate or proposition p in a context (e.g. theory) T is

$$\mathit{Mean}_T(p) = \langle \mathcal{S}_T(p), \mathcal{R}_T(p) \rangle .$$

Note the following points. First, meaning is contextual. Thus "triangle" does not have the same sense in synthetic geometry as it does in trigonometry, which – unlike the former – is a metric geometry, even though both have the same referents. Likewise "set" has different meanings in alternative set theories – i.e. these define different notions of set. This systemic view of meaning is at variance with both the semantical individualism upheld by Frege and Brouwer (every proposition has an intrinsic meaning) and Quine's semantical holism (the meaning of a construct is determined by the totality of theories, formal or empirical, where it occurs).

Second, in keeping with the conceptualist thesis adopted in Sect. 1.1, we say that the reference class of a mathematical predicate or proposition is a set of conceptual objects, i.e. of objects belonging to some conceptual system in either of the two senses elucidated a moment ago. For example, plane trigonometry refers to all the triangles on the Euclidean plane – mathematical triangles to be sure, not physical ones formed e.g. by intersecting sticks or laser beams.

A moment ago we stated that mathematics studies conceptual systems ("structures"). However, this is only a necessary condition: philosophy and the history of ideas too study conceptual systems, such as cosmologies and

mathematical theories. What makes the mathematical study of conceptual systems unique is that (*a*) it is *purely conceptual* (i.e. does not make essential use of any empirical data or procedures) and it involves, at some point or other, (*b*) *positing or conjecturing the laws* (general patterns) satisfied by the members of those conceptual systems, as well as (*c*) *proving or disproving conclusively* some such conjectures (Mathematical proofs are perfectible, but they are conclusive.) There are shorter and more beautiful definitions of mathematics, e.g. "mathematics is the *science of the infinite*" (Weyl 1949 p. 66). However, mathematics is too complex a subject to be adequately characterized by a single short phrase; for example, Weyl's definition overlooks finite mathematics.

(Rigorous proof by deduction, unlike rigorous refutation by counterexample, is distinctively mathematical. For this reason proof is often regarded as the heart of mathematics. But this is like saying that the heart of physics is hypothesis testing. Checking – proving theorems and disproving conjectures – may certainly take up most of the time of a mathematician, but it is not more important than looking for new problems, restating old problems in more suitable terms, conjecturing theorems or algorithms, building theories, or using them for solving problems. After all, most theorems must be hypothesized before they can be proved, and usually the motivation for conjecturing them is the need to solve some problem. In short, proof is certainly distinctive of mathematics, but there is more to doing mathematics than proving or disproving.)

We may then define *contemporary pure mathematics* as the investigation, by conceptual (a priori) means, of problems about conceptual systems, or members of such, with the aim of finding (inventing or discovering) the patterns satisfied by such objects – a finding justified only by rigorous proof. On the other hand we define *contemporary applied mathematics* as the investigation of problems arising in factual science, technology, or the humanities, with the help of constructs belonging to pure mathematics. (Occasionally the applied mathematician may have to supply such constructs himself, but to him they are means, not ends.)

Applied mathematics is then distinguished from pure mathematics by (*a*) the *source* of problems, which is extramathematical in the former case and internal in the latter; (*b*) the *ultimate referents*, which are real things in the case of applied mathematics, and constructs in the other case, and (*c*) the *aim*, which is to help some nonmathematical discipline in the first case, and to advance pure mathematics in the second. (More on the pure/applied distinction in Sect. 4.)

In characterizing mathematical research we have stressed that it involves proving. Now, a proof is a valid reasoning, or argument, from a set of premises to a conclusion, with the help of rules of deductive inference such as, e.g., the modus ponens and universal instantiation. However, not all proofs are alike: whereas some are rigorous, others are informal. And whereas some theorems are provable in a manageable number of steps, i.e. within a lifetime, others are not. In other words, there are *degrees of rigor*, hence of provability or theoremhood. Mathematicians reach for maximal rigor but sometimes they have to settle for less. In such cases they may find consolation in the fact, well known to historians, that rigor, far from being absolute, is time-bound. Thus, although Euclid's theorems are correct, all of his proofs of them can be improved.

Whatever the rigor of the proof of a formula, we may say that the proof *justifies* the formula. We may also say that the proof shows the formula to be *formally true*, or a *truth of reason*, to distinguish it from a truth of fact. As Grassmann (1844, Einleitung) noted long ago, whereas the factual (or "real") sciences find truth in the correspondence between thought and external objects, formal truth consists in the matching among reasoning processes: it is a thoroughly internal affair.

But if we wish to avoid confusing truths of reason with truths of fact – a confusion generating the illusion that a single theory can account for both – we may as well dispense with the notion of formal truth. (The illusion was dispelled in Vol. 2, Ch. 8, Sect. 2.) This tactics is advocated by a number of mathematicians (e.g. Bourbaki 1970) and philosophers – namely the fictionists or conventionalists. It is part and parcel of the conventionalist philosophy of mathematics, which boils down to the thesis that all mathematical formulas are conventions, so that they can be rich or poor, deep or shallow, useful or useless, elegant or clumsy, but never true in the same sense as some of the formulas or chemistry or sociology. We shall return to this subject in Sect. 5.2. Suffice it to say here that the non-truth view of mathematical truth contains a grain of truth.

The last problem in our agenda in whether mathematical objects are discovered or invented. Idealists, such as Plato, Bolzano, and Frege, hold that all ideas are already in existence somewhere, so all we can do is discover them – with hard work and luck to be sure. One good reason for holding this view is that, when constructing a mathematical object, or a theory concerning such an object, we need not posit all of its properties. In fact, when constructing a concept we need posit only its generating properties: we discover the rest as we study it. Likewise when constructing a

theory we need postulate only some propositions: the rest can be deduced as we work out the theory; or, if any additional postulates are needed, we can supply them as we proceed. In short, we invent the assumptions and discover their consequences.

So, there is evidence that mathematicians do discover. Moreover we can explain how this comes about, namely with the help of our previous assumption that mathematics is about (conceptual) systems. Indeed, it is characteristic of systems, unlike mere aggregates, that their components and properties are interdependent (lawfully inter-related). Hence, knowing some of them we can infer (discover) the others. This explains the well known fact that some mathematical results are discovered independently by several investigators. But it does not explain why others are the brain children of a few exceptional individuals.

The evidence for mathematical discovery does not suffice to justify Platonic idealism, for the following reasons. First, before we can discover that A entails B, someone must have posited A, and such positing is an invention, not the discovery of something that is already "there". Second, there is no evidence for the thesis that ideas, like mountains, are self-existing. On the contrary, there is plenty of evidence from physiological psychology that ideation is a brain process. (Recall Vol. 4, Ch. 4, or Vol. 5, Ch. 1.). Third, if mathematical ideas were already "there" (where?), up for grabs, mathematical research could proceed in a social vacuum, since productivity would depend only on individual talent. But this is not so: mathematical research is always done in some community of inquirers who inherit a cultural tradition, because mathematicians need to learn from others and they need to be stimulated and controlled by others. (Recall Sect. 1.1.)

In conclusion, in mathematics there is invention as well as discovery. We invent when positing (assuming), and we discover when finding out what our posits commit us to (logically). Think of Cauchy's famous integral formula: the value of an analytic function at an arbitrary point inside a region bounded by a closed continuous curve is determined by the totality of values of the function on the boundary: See Figure 1.1. That is, once the border and the values of the function on it have been posited, there is no room left for further invention: the interior is rigidly determined by those boundary conditions. All we seem to be doing, when using Cauchy's formula to compute a value of the given function at an interior point, is to discover some eternal Platonic idea dwelling in that domain. However, such a discovery is still a creation: the newly calculated value was *nowhere* before

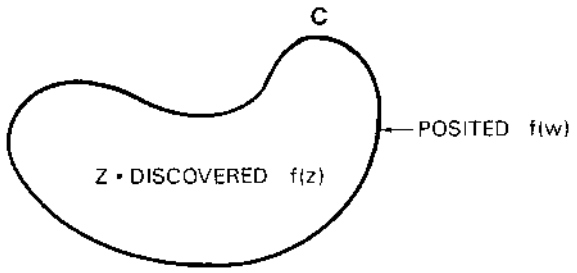


Fig. 1.1. The Cauchy integral formula. Positing all the values of an analytic function f on the curve C determines uniquely the value of f at an arbitrary interior point z:

$$f(z) = (1/2\pi i) \int_C \frac{f(w)\, dw}{w - z}$$

The positing is an invention, the calculation a discovery, and both are creative acts. Moreover they are original creations, though possibly modest ones, if nobody else performed them previously.

it was computed. All there was is a network rigidly determined by the boundary conditions: these are the generating components of the network and the only ones known beforehand. The remaining components are discovered thanks to that marvelous invention, Cauchy's integral theorem.

Whether invented or discovered, an original mathematical contribution is a *creation*: it brings to light something we did not know before. (Recall Vol. 5, Ch. 6, Sect. 1.2 on the creative power of deduction.) Of course, logicians tend to think that all the infinitely many components of a theory are born the moment the postulates are laid down. But this is just a *façon de parler*: a theorem that has not been proved, let alone guessed, is nowhere: it does not exist except potentially. What happens is that, for mathematical and philosophical purposes, we must *pretend* that such objects exist (formally), i.e. that they are components of a theory known only partially to us. In other words, we must distinguish between a construct and our knowledge of it, and feign that the construct exists (formally). A few examples will show why the distinction and the pretense are necessary – *pace* mathematical intuitionism.

Example 1 A hypothetico-deductive theory is composed of infinitely many propositions: logic "generates" them all out of a finite set of premises. We can get to know only a finite subset of the totality of formulas of a theory. Yet if we know the postulates of the theory we can study certain

global properties of it, such as consistency and completeness. *Example 2* A formula that holds for every member of an infinite set may be assumed or proved even though nobody is capable of thinking (hence knowing) every single instance of the formula. *Example 3* (Fréchet 1946). A rational number is defined as the ratio of two integers even if, given a particular number, we may be unable to find two integers that determine it (as a rational number). Thus we distinguish definitions from criteria, tests, and rules. Otherwise we would have to admit either that there are numbers which are neither rational nor irrational, or that there are gaps in every set of rationals.

In short, when doing mathematics (or metamathematics) we proceed *as if* we were Platonists: we feign that mathematical objects, and the formulas about them, exist on their own. This pretense entails our distinguishing mathematical statements from knowledge claims or disclaims, such as "We know how to construct, prove, or recognize x" – which is about knowing subjects and therefore belongs in epistemology or psychology. However, we are already encroaching on the territory of the following section.

2. MATHEMATICS AND REALITY

2.1. *Conceptual Existence*

Throughout this *Treatise* we have distinguished constructs, such as concepts and propositions, from factual items, such as concrete things and their states. Thus we have held that constructs have peculiar – mathematical and semantical – properties that factual items lack. (See e.g. Vol. 1, Ch. 1.) But at the same time we have contended that all constructs are created by rational animals and, more precisely, that they may be construed as equivalence classes of brain processes of a certain type. (See e.g. Vol. 5, Ch. 1.) Therefore our construct/fact dichotomy is methodological, not ontological. That is, we postulate that there is but one real world which is composed exclusively of factual (concrete, material) entities, some of which – people among them – are capable of creating constructs. (For the notion of a construct see Vol. 1, Ch. 1, Sects. 1.2 and 3.1; for mind-body monism, see Vol. 4, or Bunge 1980a, Ch. 5; for the construal of a construct as an equivalence class of brain processes, see Vol. 5, Ch. 1, or Bunge 1981a.)

Now, distinguishing constructs from factual items amounts to distinguishing *formal* (conceptual, ideal) *existence* from *factual* (concrete, material) *existence*. Thus we say that, whereas numbers exist formally, electrons exist

factually. One way of elucidating this difference is by stating that, whereas numbers are members of the collection of constructs, electrons belong in the collection of material objects. An equivalent way is to introduce two different existence *predicates*: conceptual (or formal) and material (or real). These predicates, rather than the allegedly all-purpose "existential" quantifier $\exists$ of logic, should be employed when discussing ontological matters. (See Vol. 3, Ch. 3, Sect. 4.3, or Bunge 1981a.) Unless these two existence concepts are distinguished and rendered precise, expressions such as 'there are no abstract entities' (found e.g. in Field 1980) are so ambiguous that they make hardly any sense. Worse, failure to draw the distinction may lead to asserting that mathematics and science are about nonexistents (Routley 1980).

Our next task is to characterize more precisely the notions of real and formal existence. We take it that the keys to real existence are *absoluteness* and *mutability*, whereas those to formal existence are *context-dependence*, *immutability*, and *conceivability*. Concerning absoluteness: whereas real things exist absolutely, every construct exists in some context or other, e.g. by fiat or by proof in some theory. For example, the natural numbers exist (formally) in number theory but not in lattice theory. On the other hand electrons and cells, mountains and forests, brains and societies, do not exist relative to some context only to vanish in another: they exist absolutely. (Caution: although things exist absolutely, certain properties, and consequently the changes in them, are frame-dependent. See Vol. 3, Ch. 5, Sect. 2.5.)

As for mutability, the difference between the two types of object is this: whereas material things are changeable, constructs are not. Thus photons keep moving as long as they exist, whereas numbers do not budge. Therefore we attempt to establish the true equations of motion for photons, not for numbers. (The equations themselves, being constructs, do not change. But of course we do change when thinking of alternative equations.) The converses too are true: whatever is changeable exists really (materially), and whatever is immutable exists only formally (conceptually). True, the derivative of a function is sometimes described informally as "the rate at which the function is changing". But this is only a hold-over from 18th century mathematics. Functions do not change although they may be made to represent changing properties of concrete things.

(To put it in the state space language adopted in Vols. 3 and 4: whereas material objects can be in at least two different states, formal objects are always in the same state – which is tantamount to saying that there is no

point in assigning them a state. Shorter: material objects can be assigned state spaces, formal objects cannot. See Bunge 1981a.)

Mathematical intuitionists agree that mathematical objects do not exist by themselves but come into existence as they are intuited or constructed. But they add that, since intuition and construction are processes, what they produce is just as temporal as a material object. (This is why they reject the concept of actual infinity as unintelligible: "we can arrive at the notion of infinity in no other way than by considering a process of generation or construction which will never be completed": Dummett 1977 p. 56.) That mathematical research is a process, is obvious but irrelevant to the question of the nature of its products. We must distinguish (though not separate) the activity from its product, e.g. research from its finding, because the former is personal whereas the latter may be universal. (Recall Vol. 5, Ch. 2 on the distinction between cognition and knowledge.) Moreover in standard ("classical") mathematics we pay no attention to the psychology of research and *feign* that all the admissible mathematical objects are ready made: we thus distinguish mathematics from the creation or learning of it. There is nothing to prevent us from inventing such a fiction precisely because mathematical objects are *entia rationis* quite unlike material objects, which are forever in a state of flux and satisfy factual (e.g. physical) laws. (More on mathematical fictionism in Sect. 5.2.)

To put it into other words: When creating or discovering mathematical objects we fashion them *as* atemporal items. In other words, mathematical propositions and, in general, truths of reason – unlike truths of fact – are tenseless: i.e. the concept of time fails to occur in them. Thus we distinguish the mathematical proposition "π is transcendental" from the factual proposition "π was shown to be transcendental one century ago". If intuitionists were consistent they would introduce the concept of time as a basic (primitive) concept of mathematics. (As a matter of fact once Brouwer did try do do just this.) But this would lead them into contradiction, for such a concept must be elucidated with the help of prior notions, such as those of set and function, not to speak of those of event and reference frame. (Recall Vol. 3, Ch. 6, Sect. 3.) By avoiding this inconsistency, intuitionists behave inconsistently.

Finally, the conceivability condition is this: for something to be a construct it is necessary and sufficient that it may be conceived by someone. (On a realistic epistemology like ours it is not necessary for a real thing to be conceivable in order to exist. The universe existed before it contained animals capable of conceiving it. On the other hand there were no mathe-

matical objects before the emergence of animals capable of conceiving them.) In other words, we stipulate that x is a *construct* if, and only if, there exists (really) an animal capable of conceiving x as a conceptual system or a member of such. (Recall our definition of a conceptual system in Sect. 1.2.)

Our preceding convention allows not only for mathematical constructs but also for mythological ones. We are far more demanding when it comes to mathematical existence. We demand that it be defined in or by a consistent theory dealing with exact concepts. It follows that the constructs typical of fuzzy or inconsistent theories are devoid of mathematical existence: they do not differ essentially from mythological characters. (Constructivists are even more demanding: for them only constructive existence, and moreover provided it is ultimately reducible to positive integers, is legitimate. See Sect. 5.) We shall come back to this point in a short while.

In sum, whereas real existence is absolute (context-free) and is characterized by mutability, formal existence is relative (context-dependent) and is characterized by immutability and conceivability. Hence in our view Plato was right in holding that ideas are immaterial and immutable, wrong in believing that they exist really by themselves. And Hegel was wrong not only in asserting that ideas are self-existing but also in holding that they are characterized by self-activity: that they go by themselves through the three dialectical stages (thesis, antithesis, synthesis) propelled by contradiction. Only real (material) entities are changeable.

So much for formal and material existence in general. Let us now turn to particular existence statements, such as "There are infinitely many lines passing through any given point in three-dimensional space". Such existence statements are just as essential in mathematics as real existence statements are in factual science. In the latter we certify or at least conjecture explicitly the existence of entities of a certain kind before proceeding to accounting for them in any great detail. (Characteristically, ideology and pseudoscience describe objects the existence of which has not been or cannot be established.) And in ordinary mathematics one postulates or proves the existence of objects of a certain kind before proceeding to conjecture or prove the laws they satisfy. (In constructive mathematics one must start by constructing the objects concerned with the help of explicit procedures such as computations.) Indeed, the antecedent of a general mathematical law is (tacitly or explicitly) an existence statement, omitted only when taken for granted. The simplest such law is of the form: "If x exists, then: If x has property F, then x has also property G". Omission of

the cautious existence clause may result in laborious manipulation of ghosts. (If existence is not presupposed or proved, some system of free logic must be assumed, or else the formal existence predicate introduced by Fourman 1977 must be used.)

Every existence statement can be construed as a statement about the membership of the object(s) concerned in some set, class, or category specified by an exact consistent theory. *Example 1* The empty set axiom, namely "There is a set without members", is replaceable with: "The category of sets contains one set without members". *Example 2* Euclid's theorem about the existence of infinitely many prime numbers can be recast thus: "The set of prime numbers is an infinite subset of the set of natural numbers". *Example 3* Any theorem asserting the existence of real functions solving differential equations of some type can be translated into the assertion that the set $\mathbb{R}^{\mathbb{R}}$, where $\mathbb{R}$ is the real number system, contains functions satisfying the given equation.

Our thesis can now be restated as follows:

DEFINITION 1.1 If x is a construct, then x *exists mathematically* $=_{df}$ For some C, C is a set, class, or category, such that (i) x is in C, and (ii) C is specified by an exact and consistent theory.

(Of course the construct may have been conceived before being included in a set, class, or category described by a theory. But this psychological or historical circumstance is irrelevant to our concern, which is the mathematical legitimacy of the construct in question.)

What about the containing set, class, or category itself? In some cases it exists (formally) by virtue of being contained in a larger object, so we can answer the question 'Where (in which set, class, or category) is x?' But if the more comprehensive object happens to be the totality of constructs, then the question makes no more sense than the question 'Where is the physical universe?'.

By shifting the context from mathematics to some other field of knowledge, existence problems and existence statements (such as theorems) can be analyzed differently. For example, the formula "$(\exists x)Fx$", usually read 'There are F's', can be assigned, among others, the following interpretations:

(i) *ontological*: some real things possess the property represented by predicate F;

(ii) *psychological*: for some rational being y, and some time z, y thinks at time z that some objects possess property F;

(iii) *pragmatic* (or intuitionistic): some object(s) can be found or constructed with property F.

Clearly, none of these alternative readings is strictly mathematical. The ontological interpretation, legitimate in any factual discipline, calls for an empirical investigation to determine whether, in fact, there are material things possessing the property represented by the concept *F*. The second interpretation, legitimate in psychological contexts, stretches far beyond mathematics for making a claim of real existence; moreover, unlike the original statement, the psychological analysans is irrefutable. Finally, the pragmatic or intuitionistic interpretation – which is often adopted in the classroom and in computer science – produces a proposition that may not hold with the means available to the individual who adopts it. In any event all three interpretations go beyond the standard mathematical interpretation of the formula "$(\exists x)Fx$", which is simply this: *Some* constructs possess property *F*. (Equivalently: The set, class or category determined by *F* is nonempty.)

(By the way, what holds for the nonmathematical interpretations of "$(\exists x)Fx$" holds, *mutatis mutandis*, for the interpretation of any other mathematical formula. In doing or teaching mathematics we often indulge in extramathematical interpretations. Although such license can be heuristically or didactically effective, it can also be seriously misleading. For example, if we were to read the identity "12 = 7 + 5" as "12 can be decomposed or analyzed into 7 and 5", then it would cease to be identical with "7 + 5 = 12", and so the '=' sign involved in the two equations would not designate the identity relation, which is symmetrical. Likewise, if we interpret the fundamental theorem of algebra in pragmatic terms, as asserting that *we can find n* not necessarily different roots of a polynomial equation of degree *n*, then every time we fail to find such roots – as will often be the case for $n \geq 5$ – we may feel entitled to conclude that we have refuted the theorem. In short, within pure mathematics only mathematical interpretations are legitimate, all others are spurious. Nonmathematical interpretations of mathematical formulas are legitimate only in extramathematical contexts, such as physics and economics.)

The problem of mathematical existence is widely felt to arise only in relation with infinite sets. Thus intuitionists have no difficulty with finite sets but take extraordinary precautions – to the point of adopting a logic of their own – when it comes to infinite sets. And formalists concede that "Infinite totalities do not exist in any sense of the word [...] Nevertheless [...] we should act *as if* infinite totalities really existed" (Robinson 1965, p. 230). In our view all sets, whether finite or infinite, and indeed all constructs, pose the same philosophical problem and they exist in the same way, namely

formally (or conceptually or ideally) – provided of course they are well defined. In other words, from an ontological point of view there is no difference among constructs: the modest number 0 and the entire "real" line exist in the same fashion, namely formally. None of them is physically real, and every one of them (except the totality of constructs) exists as long as it can be conceived by someone as being a component of some set, class, or category.

Of course conceivability, a necessary property of constructs, may pose problems even with regard to "small" mathematical objects. However, such problems are solved the minute we recall that, far from being stray and lawless, mathematical objects are systemic and lawful. An example will show why this is so. Consider an arbitrary 100 digit number n, and form the enormously greater number $n!$ (read 'n factorial'), i.e. the product of all the positive integers up to n. Multiply the result by $n + 1$, to obtain $(n + 1)!$. Since $(n + 1)! = (n + 1)n!$, $(n + 1)!/n! = n + 1$. A computer may handle the RHS but perhaps not the LHS of this identity in the case we are considering. Yet no mathematician doubts the (formal) existence of $n!$, or the quotient of the two. All three are well defined objects: they are defined by the laws (axioms, definitions and theorems) of number theory. And this is all that matters in mathematics: systemicity and lawfulness.

An obdurate empiricist might rejoin that he is prepared to admit large numbers, sets, etc., provided they are finite, for such constructs can somehow be imagined by extrapolation from smaller objects of the same type. But he refuses to countenance infinite numbers, sets, etc., because these are inaccessible to imagination, which (he believes) is always tied to sense experience (or to action in the case of pragmatism). In particular, the empiricist and the pragmatist may ask: How can you assert that there are infinitely many integers, if counting them all would take an infinite time, and would thus be impossible? (This question was posed by my daughter Silvia at age 5.)

The mathematician's answer to the above question is this. Although we cannot produce *every* single integer, *in principle* (though perhaps not in actual psychological fact) we can produce *any* integer by adding 1 to its predecessor. (Note the differences between "in principle" and "in actual fact", and between "every" and "any".) In general, we produce and "handle" infinities by using the finitary laws – e.g. of iteration or recursion – that they satisfy. For example, the sum of the powers of $\frac{1}{2}$ can be calculated as follows. Set $S = \frac{1}{2} + (\frac{1}{2})^2 + (\frac{1}{2})^3 + \ldots$ Multiply both sides by 2, to obtain $2S = 1 + \frac{1}{2} + (\frac{1}{2})^2 + (\frac{1}{2})^3 + \ldots = 1 + S$, whence $S = 1$.

In performing the preceding calculation we did not have to think of every member of S. We thought of the infinite series S as a whole defined by the law that generates it, a law that is finitary. (Actually we should have started by checking whether the sequence $\frac{1}{2}, \frac{1}{2} + (\frac{1}{2})^2, \frac{1}{2} + (\frac{1}{2})^2 + (\frac{1}{2})^3$, etc. converges to a limit.) We do not have to suppose, with the idealist philosophers, that the human mind has direct access to infinity. All we have to suppose is that the infinity in question is systemic and lawful. To put it in psychological terms: When faced with a (soluble) mathematical problem, whether or not it involves infinities, our task is to use or construct a finite number of functions pairing the inputs (data) to the outputs (solutions).

It might be thought that the preceding considerations hold only for specific sets, such as the real line and the complex plane. It might be doubted that our views hold also for abstract or nondescript sets, such as those occurring in the formula "$A \cup B = B \cup A$", where A and B are (or rather designate) sets. That is not so: if a set is abstract, i.e. nondescript or faceless, we handle it as a totality, assuming that it satisfies the algebra of classes. Systemicity and lawfulness legitimate our dealings with abstract sets, whether finite or infinite, denumerable or non-denumerable. The same holds, *mutatis mutandis*, for all the other mathematical objects.

In short, we must distinguish conceptual from material existence. Whereas to be a material thing is to be able to change in some respect, to be a construct is to be conceivable; and to be a mathematical construct is to be *conceivable consistently*. And, whereas material things are real, constructs are not; in particular, mathematical constructs are fictions. Consequently our materialistic ontology (Vols. 3 and 4), which refers to material things, does not hold for constructs. And our epistemology (Vols. 5 and 6) applies to the constructs purported to represent material things, but not to the rest: it is realistic with respect to the former, and fictionistic with regard to the constructs of logic and pure mathematics. (See Sect. 6 for the consistency of this dualism.)

2.2. *Mathematics and Reality*

How does formal (conceptual) existence relate to real (material, concrete) existence? We have already met one such relation, namely that between creators and creatures, i.e. inquiring animals and their conceptions. We have said that all constructs are created by higher animals: no thinking animal, no construct. (This is of course a denial of Platonism.) More precisely, we have characterized a construct roughly as an equivalence class of neural processes. This characterization accounts for the fact that, re-

gardless of the ways in which different animals (or a single animal at different times) think of a given construct, they always think of the same construct despite their physiological differences.

(It might be objected that our characterization of a construct presupposes the notions of relation and of class. True, but this does not involve identifying constructs with brain processes. It is not that the processes themselves rely on such notions: they occur, and are similar in certain respects, whether we happen to know it or not. Likewise the members of a given chemical, biological or social species are similar to one another, and such similarities are objective, provided the species concept is well defined, even though every species is a set, and therefore a concept rather than a concrete thing. The notions of class and relation, as well as many other concepts, are required to account for reality but they are not themselves real. Our view raises interesting problems: see Torretti 1982.)

Another possible relation between formal and real existence is the one obtaining between a mathematical system and a physical (or biological or social) one. In other words: How is mathematics related to reality? This is of course a subproblem of the general question: What is the relation between ideas and the external world? If we look for an answer in the history of ideas we may be easily misled for, by conveniently disregarding counterexamples, we are likely to find cases confirming almost any of our philosophical prejudices. Thus from the fact that *some* mathematical ideas have originated in practical concerns, or have ended by being used in science or technology, one might be tempted to "conclude" that *every* mathematical object represents some trait of reality – the empiricist, pragmatist, and vulgar materialist thesis; or that every thing is identical with, or at least an imperfect copy or realization of, some mathematical object – the objective idealist thesis.

Both "conclusions" are wrong, as shown with the help of our theory of reference (Vol. 1, Ch. 2). Thus the laws of the algebra of classes – such as "$A \cup A = A$" – hold regardless of the nature of the members of the classes concerned. And the axioms governing the relation "less than" are independent of the nature of the elements of the set on which it is defined. Mathematics is *ontologically noncommittal*, and this is why it can be employed as a tool in constructing theories representing things of many different kinds – or none. Indeed the same mathematical systems ("structures") are likely to occur and recur in a great many different research fields, each time together with a different interpretation. Thus the real line can be used to represent a spatial direction, time, the degrees of a "quantity" such as energy, and much else. However, such interpretations (or semantic

assumptions) are not part of pure mathematics: they are part of factual theories. (Recall Vol. 2, Ch. 6, Sect. 3.) The propositions in pure mathematics are about purely conceptual objects such as sets and functions.

If mathematics does not represent the world, if it is not the most general science of reality, then in particular it does not account for change. Shorter: mathematical objects are timeless. (But of course mathematical activity is temporal – like any other process.) Any mathematical description of real change involves some semantic assumptions whereby certain mathematical objects are assumed to represent nonmathematical objects such as places, times, velocities, or some other properties of real things. For example, the formula $\ulcorner y = \frac{1}{2}ax^2 \urcorner$ will represent, in one context, the distance travelled by a freely falling body (acceleration a) after time x; in another, the average power of a circuit (resistance a) through which alternating electric current of intensity x flows. Change occurs in the things represented by the mathematical constructs, not in the latter.

True, a moving body is sometimes said to integrate its equations of motion; a propagating wave, to integrate its field equations; a chemical reaction, to integrate its chemical kinetics equations; and the neurons in the visual cortex, to iterate certain Lie operators. But all these are metaphors. What is meant is that motion, chemical reaction, vision, or some other process, is *described* by certain mathematical operations. We do not *identify* processes with mathematical operations but assume that the latter can correctly *represent* the former. The representative exists conceptually (formally), the represented materially (really). The fact that in describing change we often employ the same mathematical objects, e.g. second order ordinary or partial differential equations, suggests that we muster only a somewhat limited conceptual repertoire. Nature does not care which concepts we employ to account for her. But we do care.

The contrast between immutable mathematical constructs and changing reality has baffled many thinkers for many centuries. Thus Plato believed that, since mathematical objects are immutable and perfect, and mathematical knowledge (unlike our knowledge of nature) is certain, it would never be possible to have a science of nature, which is always in flux and therefore imperfect. Only mathematics is science proper (*episteme*); everything else is opinion (*doxa*). "As being is to becoming, so is truth to belief" (*Timaeus* 28). Bergson (1907) too believed that intelligence, in particular science, is impotent to capture change and, in particular, novelty. And, just as Plato had stated that we can imagine myths about the world, but are incapable of building a science of nature, so Bergson claimed that intuition can grasp the mutability and creativity that elude intelligence.

More recently the same problem has been a philosophical motivation for the construction of an important new theory in the foundations of mathematics: topos theory. And some leading mathematicians have recently come to believe that set theory cannot be the correct foundation of mathematics because sets have an unchanging membership, and so they cannot adequately represent the mutability characteristic of reality, which, it is held, mathematics should strive to reflect. (See Lambek 1982.)

Our rejoinder to those who puzzle the contrast between the immutability of mathematical constructs and the mutability of reality is that the very nucleus of every advanced science is a set of equations of change of some sort of other, which equations involve unchangeable mathematical objects such as functions. For example Maxwell's equations, which do not propagate, serve to represent the propagation of electromagnetic waves; and the equations of population genetics serve to represent an aspect of the evolution of biopopulations. The reason for this ability of unchanging mathematical objects to represent change is, to repeat, that in factual science the mathematical objects concerned are taken together with semantic assumptions ("correspondence rules"). The latter specify what are the referents of those constructs and what properties of them they represent. Obviously, an examination of pure mathematics will not yield such semantic assumptions because pure mathematics is ontologically noncommittal. Only a semantic analysis of factual theories, e.g. in chemistry or in biology, will yield them. (See Vols. 1 and 2 for the concept of a semantic assumption and examples.)

To understand how scientists manage to account for change with the help of unchanging mathematical constructs, it may help to consider a few instances. *Example 1* Call L Archimedes' law of the lever, an algebraic expression involving symbols interpreted in mathematics as numbers, and in physics as values of distances and weights. (Recall Sect. 1.2.) The condition of nonequilibrium or movement of such a system is obviously the negation of L. But note that *not-L* does not involve the notion of time. *Example 2* Consider a continuous function f from reals to reals: no change so far. The function can be made to represent a continuously varying property of some real thing if $\mathbb{R}$ is made to represent time, and the value $f(t)$ of f, for t in $\mathbb{R}$, the value of the property concerned at time t. Think of the elongation $\sin wt$ of a vibrating mass point.

Example 3 To represent a collection of changing membership, such as the population of Paris, we can introduce the notion of a variable (or time dependent) collection (Vol. 3, Ch. 5, Sect. 2.5). A variable (or time depen-

dent) collection S_t is definable as the set of all the individuals possessing the given (defining) property P at a given time t, i.e. $S_t = \{x|Pxt\}$, where $t \in \mathbb{R}$ is interpreted as an instant. (Think of Pxt as "person x lives in Paris at time t".) The notion of an invariable collection can now be obtained by taking the union of all the S_t over time, i.e. $S = \bigcup_{t \in \mathbb{R}} S_t$. And if necessary we can define a time dependent membership relation $\in_t$ through the condition:

$$b \in_t S =_{df} b \in S_t.$$

Thus to say that Aristotle *is* human, or belongs to the human species S (invariable membership), amounts to saying that Aristotle belong*ed* to the collection of humans who *were* alive at the time of Alexander. In short, the notion of a variable collection is definable in terms of the mathematical concept of a set and the ontological (or protophysical) concept of time. (However, it is also possible to adopt the notion of variable collection as undefined.)

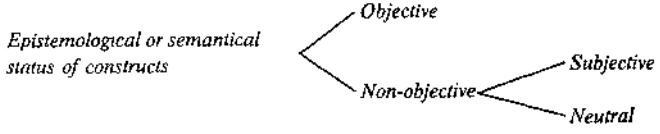
The preceding examples illustrate our thesis that, although pure mathematics is ontologically noncommittal, it provides a conceptual framework which, when enriched with the suitable semantic assumptions, can be made to account for our restless universe. In the last two cases the (or rather a) concept of time, which is alien to pure mathematics, occurs essentially, in Example 2 as the independent variable, in Example 3 as an index. It was taken as a primitive concept to be elucidated elsewhere, namely in ontology or in protophysics. However, any exactification of any concept of time, or of any other factual concept for that matter, is bound to employ some mathematical (and also some nonmathematical) concepts. (See Vol. 3, Ch. 6, Sect. 3.). Hence the notion of a variable collection, defined in Example 3, cannot occur in the foundations of mathematics except as an undefined concept. Shorter: We need the a priori science of mathematics to build the a posteriori sciences of the real world. (More on this in Sect. 4.1.)

Although mathematics is not semantically objective, for it does not represent the external world, it is undoubtedly objective in some other sense, in which art and religion are not. Thus we all agree that the rational numbers are denumerable, that the rotations about a fixed point constitute a group, and that some differential equations can be integrated by the method of the Laplace transform. Moreover such agreement is not merely a matter of fashion or arbitrary convention: it is a result of reasoning. How can that be? The answer is simply that, once certain assumptions have been adopted, we are rationally committed to admitting their logical consequences. Moreover such assumptions are not stray but form systems (theories). So, the ob-

jectivity of mathematics consists in the systemicity and lawfulness of its objects, not in that mathematics is a sort of universal physics. Shorter: mathematics is *methodologically objective*, not semantically or epistemologically objective.

Does our denial that mathematics is semantically or epistemologically objective entail that it is subjective? Certain formulations of mathematical statements do cause this impression. For example, Zermelo's principle is sometimes stated "Every set *can be* well ordered", and Euclid's 5th postulate is often stated "Through any point outside a given line *it is always possible to draw* a single parallel". However, these formulations are not strictly mathematical: they are pragmatic interpretations of "*There is* a well ordered set containing the elements of any given set", and "*There is* a single parallel through any point outside a given straight line". (In both cases 'there is' designates the concept of conceptual existence.) None of these statements refers to subjective experience or to action: they only assert the formal existence of certain mathematical constructs.

How is it possible for something, in particular mathematics, to be *neither* objective *nor* subjective in an epistemological or a semantical sense? The reason is that *objective/subjective* is not a dichotomy, for *tertium datur*, namely epistemological (or semantical) *neutrality*. Indeed, the correct dichotomy is objective/non-objective, where "non-objective" equals the exclusive disjunction of subjective and neutral. I.e.,



From an epistemological (or semantical) viewpoint mathematics is neither subjective (intuitionism) nor objective (Platonism and dialectical materialism) but *neutral*, because it is neither about subjective experience nor about an autonomously existing world. Of course mathematical creation presupposes the existence of creators, i.e. living mathematicians working in a favorable culture. (No mathematics without mathematicians: mathematics will end with the last mathematician.) But the point is that, although mathematics is created by real beings, it is neither independently real nor, by itself, representative of reality. Yet nothing prevents us from *pretending* that mathematical objects exist in a *sui generis* fashion (i.e. formally), and everything encourages us to use mathematics in our study of reality.

Mathematical objects are then ontologically on a par with artistic and

mythological creations: they are all *fictions*. The real number system and the triangle inequality axiom do not exist really any more than Don Quijote or Donald Duck. Nor is the difference epistemological, for some mathematical constructs, just as some artistic and mythological fictions, are idealizations of real things or features of real things. Nor, finally, is mathematics distinguished from art and myth by its certainty, even though the yearning for final certainty has often been a powerful motivation for mathematical research, particularly in the foundations of mathematics. But the successive crises in the latter, the current multiplicity of inequivalent set theories, and the continuing controversies over the axiom of choice, have taught us that final certainty is a mirage. True, mathematics is more certain than factual science, which is in turn more certain than other research domains. Still, mathematical research is not error-free. True, mathematics has foundations, but the latter are not unique and they are not immutable.

The crucial differences between mathematical fictions and all others are found elsewhere. Unlike other fictions,

(i) mathematical objects, though devoid of factual reference, are not totally free inventions, let alone lies or products of self-deception: they are constrained by laws (axioms, definitions, theorems); consequently they cannot possibly behave "out of character" – e.g. there can be no such thing as a right angle equilateral triangle, whereas even mad Don Quijote is occasionally lucid, and even gentle Mickey Mouse is occasionally cruel;

(ii) mathematical objects exist (formally) either by postulate or by proof, never by arbitrary fiat;

(iii) mathematical objects are theories or referents of theories, whether full-fledged or in the making, whereas myths, fables, stories, poems, sonatas and paintings are nontheoretical;

(iv) mathematical objects and theories are fully rational, not intuitive, let alone irrational (though of course mathematical intuition is acquired with practice);

(v) mathematical statements must be justified in a rational manner, not by intuition, revelation, or experience;

(vi) far from being dogmas, mathematical theories are based on hypotheses that are given up if shown to lead to contradiction, triviality, or redundancy;

(vii) mathematical theories are linked together forming a supersystem; thus logic employs algebraic methods, and number theory resorts to analysis; on the other hand there is no such thing as a coherent system of artistic or mythological creations;

(viii) mathematics is neither subjective nor objective, but ontologically noncommittal; only the process of mathematical invention is subjective, and only living mathematicians are real;

(ix) mathematical objects and theories find application in science, technology, and the humanities;

(x) mathematical objects and theories are socially neutral, whereas myth and art often support or undermine the powers that be.

So much for the general traits of mathematics. Let us now take a somewhat closer look at our science, starting with logic, the most basic branch of mathematics and the intersection of mathematics and philosophy.

3. LOGIC

3.1. *Logic Lato Sensu*

Logic originated as the art of conversation and, in particular, argument. Eventually it developed into the theory of deduction. We shall call this restricted construal of 'logic' *logic stricto sensu* or *basic logic*. This is the way philosophers usually conceive of it. Mathematicians nowadays include in logic not only the theory of deduction but also model theory (not to be mistaken for modal logic), set theory, recursion theory, and often also category theory. (See e.g. Barwise Ed., 1977.) We shall call this *logic lato sensu*. There is an even more comprehensive construal of "logic": some philosophers have talked about 'the logical structure of the world', 'the logic of discovery', 'the logic of decision making', 'the logic of verbs', and even 'the logic of the situation'. We shall disregard these metaphorical uses of the word 'logic'.

In this section we shall tackle a random sample of philosophical problems about logic *lato sensu*. In particular we shall ask: What are predicates and propositions?, How is $\exists$ to be interpreted?, What is a model of an abstract theory?, What is the philosophical import of Gödel's incompleteness theorem?, What is all the fuss about the axiom of choice?, Are sets necessarily the building blocks of mathematics?, and What is logic?

To start with the first of the problems in our list: as we saw in Vol. 5 (Ch. 5, Sect. 2.1), *predication* or attribution is a fundamental cognitive operation. In ordinary knowledge, factual science, and technology, predication consists in conceiving predicates that are hoped to match the properties or relations observed or conjectured to hold in reality. Thus the conceptualization of ordinary experience with ponderables results in predicates such as "heavy" and "light", "heavier than" and "lighter than", "weight of a body" and "weight of a body relative to a reference frame".

Predication is sometimes sparked off by experience but it can go far beyond the latter, and indeed in two ways. Firstly, in all walks of life we employ transempirical predicates such as "weightless", "unseen", and "shoddy or expensive". Modern science and technology teem with such predicates: think of "state (of a thing)", "ionization", and "balance of payments". Secondly, mathematicians keep inventing predicates that do not correspond to any substantial properties, i.e. properties possessed by material things. Think of "predicable", "is a logical consequence of", "definition", and "consistent" – to mention only a few logical and metalogical predicates.

This brings us to an important yet generally overlooked difference between the predicates occurring in mathematics and all the others. Whereas a mathematical predicate *is* (identical with) a property of a construct, a predicate in any other field of knowledge is (rightly or wrongly) supposed to *represent* a property of some substantial (material, concrete, real) thing. (We drew this difference in Vol. 3, Ch. 2, Sect. 1.) Thus it is indifferent whether we call 'differentiable' (with reference to functions) a predicate or a property. On the other hand we must draw a difference between a property of a material thing and any of the predicates that may represent that property. First, because predicates are constructs whereas substantial properties are features of real things, hence inseparable from the latter. (Real things "come" with all their properties.) Second, because one and the same property can be conceptualized differently in different theories: think e.g. of the mass and length properties in classical and in relativistic mechanics. (Incidentally, the fact that different theories may choose different attributes to represent one and the same substantial property confirms critical realism as formulated in Bunge, 1983b. And it refutes idealism – which identifies attributes and properties – as well as naive realism, which postulates a 1:1 correspondence between attributes and properties.)

The most basic, and therefore the coarsest, formalization and systematization of predication (or attribution) is the predicate calculus. This is logic *stricto sensu*: it studies predicates and the formulas and deductive inference patterns in which they occur. (Examples of kinds of predicate: unary such as "heavy", binary such as "heavier than", and in general n-ary. Example of a formula: open such as "Axy", and closed such as "$(\forall x)(\exists y)\,Axy$". Example of a rule of inference: $Aa \vdash (\exists x)Ax$.) In turn, the predicate calculus is a refinement of the propositional calculus, which does not analyze the basic or atomic propositions, such as "a is heavier than b". Still, the predicate calculus is extremely coarse in comparison with the general theory

of functions. Thus the latter allows us to analyze formulas such as "The telephone number of x is y", and "The production of x in dollars, during the year y, was z". Morever, unlike the predicate calculus, the theory of functions allows one to distinguish between different functions with the same arguments, such as the linear function, the sine function, and the logarithmic function. Logic jumbles all of the infinitely many functions of one real variable into the binary predicate.

The notions of a predicate and of a proposition are, of course, elucidated by the predicate calculus. But a somewhat deeper study of those notions calls for the mathematical concept of a function (Vol. 1, Ch. 1, Sect. 1). Indeed, an arbitrary unary predicate P can be construed as a function (or many-one relation) from a domain A of objects to the set S of all the propositions involving P. In symbols, $P{:}A \to S$. The value of P at a in A is Pa, a member of S. Likewise, an arbitrary binary predicate R can be construed as a function from ordered pairs of individuals – i.e. from elements of the cartesian product $A \times B$ of two sets – to the set T of all the propositions involving R. In symbols, $R{:}\ A \times B \to T$. The value of R at $\langle a, b \rangle$ in $A \times B$ is Rab, a member of T.

For example, the predicate "predicable" can be meaningfully (though perhaps falsely) predicated of concepts, not of material objects. In other words, the domain of that predicate is the set of concepts. The fact that most concepts cannot be truly predicated of themselves (e.g. "even" is not even) is beside the point. The notion of truth does not occur in our analysis of a predicate. (It does occur in Frege's queer notion of a predicate as a function from individuals to truth values: see Vol. 1, Ch. 1, Sect. 1.3.)

We have clarified the functional relation between predicates (or attributes) and propositions without saying explicitly what these constructs are. Yet one answer to this question is tacitly included in the above: A proposition is a value of a predicate, and predicates are defined (implicitly and in broad outline) by the axioms of the predicate calculus. Still, this definition is psychologically and practically as unsatisfactory as the (correct) characterization of an electron as that which satisfies Dirac's theory of electrons. We would like to have an independent characterization of a proposition (and of an electron), not only to satisfy our intuition but also to be able to recognize it as such when meeting it, as well as to be able to identify propositions across the various alternative logical theories (such as the classical and the intuitionistic predicate calculi).

One possible move is to say that a proposition is an object composed of concepts, in particular predicates. This is true but insufficient, for constructs

can be combined in ways that do not yield propositions. (E.g., 'Dogs are' is a meaningless combination of meaningful words.) Something more is needed, namely a particular structure: the concepts have to be put together in a proper way. In other words, we may conceive of a proposition as a (conceptual) system composed of concepts and having a structure of a certain kind. Propositions with the same composition but different structure, such as "Dogs are animals" and "Animals are dogs", may be called *isomeric*. On the other hand propositions with the same structure but different compositions, such as "Space is divisible" and "God is omnipotent", may be called *isomorphic*. However, none of this is too helpful because it does not specify the right kind of structure, i.e. the one that combines concepts into propositions. (Saying that the structure is specified by the formation rules of the predicate calculus is quite true but not to the point, since our goal was to characterize the notion of a proposition independently of that theory.)

A second move is to resort to semantical considerations. Thus we may say that predicates are objects with a sense (or content) and a reference. True but insufficient, because in our theory propositions too have both properties. Besides, our own theories of sense and reference (Vol. 1) presuppose that we know what a predicate and a proposition are. Another possible way out is to switch from considerations of meaning to considerations of truth. As a matter of fact the most popular "definition" of a proposition is this: A proposition is whatever has a truth value. However, this characterization has two defects. One is that it, too, presupposes some knowledge of what a proposition is, at least enough to distinguish the semantical concept of truth from the concepts occurring in the phrases 'a true gem' and 'a true friend'. The second shortcoming is that the great majority of the propositions occurring outside pure mathematics are at best approximately true, at worst undecidable.

The balance of our exercise is this. First, it is true that propositions are systems of concepts, and that the structure of such systems is specified by the rules of formation of the predicate calculus. (E.g., if M and N are predicates of rank m and n respectively, and O is an arbitrary set of objects, then $Mab\ldots m$, $Nde\ldots n$, $\neg Mab\ldots m$, and $Mab\ldots m \vee Nde\ldots$ with $a, b, \ldots, m, n \in O$, are propositions.) Likewise it is true that both predicates and propositions have senses and references. Finally, it is also true that some propositions have a truth value, though not necessarily 0 or 1. However, our semantical considerations do not serve to define either predicates or propositions. What they do is, first, to *supplement* the syntactic characterization

given by the predicate calculus. Second, they serve as indicators or *criteria* of predicateness or propositionhood (pardon the neologisms). For example, if P is a construct that combines objects a and b into a construct of the form Pab, then P is a predicate of rank 2, and Pab a proposition. (Incidentally, the fact that such criteria cannot double as definitions refutes the constructivist or operationist doctrine of definition. Likewise, convergence criteria or tests presuppose rather than replace the definition of convergence.)

What happened to our desideratum of being able to identify a given proposition across all possible logical calculi? After all, (*a*) the point of building different theories of objects of a given kind is to attempt to capture in the best possible way the essential properties of such objects; but (*b*) if two constructs are defined (implicitly) by different theories (not just different presentations, e.g. axiomatizations, of one and the same theory), then they are not exactly the same constructs. (Recall from Sect. 1, 2 that constructs are characterized by their sense and reference, so that even if two theories refer to the same objects, they can have different senses, hence they describe different constructs.)

Note the difference between this problem and that of identifying the referents of two different factual theories, such as two alternative theories of the electron. In this case there is a single real object modeled now in one way, now in another; i.e., there are two different, possibly rival, concepts of the electron. But both constructs, if they are to command the attention of scientists, will have to satisfy the description of an electron as an entity with such and such mass, spin, and electric charge values. In the case of propositions we cannot resort to such empirical characterizations because propositions are *êtres de raison*, not material entities that can be manipulated in the laboratory. In this case our considerations must be intraconceptual and, more precisely, metatheoretical.

What we may do in this case is the following. We may stipulate that two theories deal with propositions (and also possibly with predicates) if, and only if, (*a*) they define (either implicitly or explicitly) operations such as "or", and (*b*) they share some tautologies, such as "If p and q then q", or some rules of inference, such as the *modus ponens*. (Caution: do not require that the principle of non-contradiction be included among the shared tautologies, for it cannot even be stated in negationless logics, and it is not assumed in the so-called paraconsistent logics. More on this in Sect. 3.2.)

The second item in our agenda concerns the proper interpretation of the *"existential" quantifier* $\exists$, which we prefer to call *particularizer* or *indefinite*

quantifier, to distinguish it from both the *universalizer* $\forall$ and the *individualizer* (or descriptor) ι. As it is well known, most logicians believe that "Existence is what existential quantification expresses" (Quine, 1969, p. 97). We have argued in Sect. 1.3 that this is a mistake: that there are two radically different concepts of existence, which $\exists$ does not distinguish: formal (or conceptual) and material (or real) existence. We have also argued (Vol. 3, Ch. 3, Sect. 4.3) that, far from exactifying either of these concepts, $\exists$ exactifies the concept of *some*. Thus, "$(\exists x)x$ is a ghost" is not to be read as "There are ghosts" but as "Some individuals are ghosts" – namely some figments of our imagination. (An alternative interpretation of "$(\exists x)Gx$" is: The formula "Gx" is satisfiable. Equivalently: Some substitution instances of "Gx" are true". Though different, the two interpretations – which occur in *Principia Mathematica* – are equivalent: Something has G if some substitution instances of "Gx" are true and conversely.)

To clarify the above consider the statements

There are wicked ghosts. (1)

Some ghosts are wicked. (2)

According to Russell, Quine and most other logicians these two statements are identical, and they (or rather it) must be formalized as"$(\exists x)(Gx \,\&\, Wx)$". In our view (1) and (2) are different: the first has an existential import, the second does not. Consequently the first step towards an adequate formalization of (1) is to rope in the existence predicate E_M defined in Vol. 3, Ch. 3, Sect. 4.3. Being a predicate, E_M operates on the individual variable x like any other predicate. The result is: $E_Mx \,\&\, Gx \,\&\, Wx$. If 'M' is interpreted as mythology, our formula reads: In mythology there are individuals both ghostly and wicked. If we wish to obtain a proposition (closed formula) we prefix the particularizer $\exists$, to obtain $(\exists x)(E_Mx \,\&\, Gx \,\&\, Wx)$, read "Some of the ghosts (that exist) in mythology are wicked". Only now have we made an ontological commitment, whereas (2) remains ontologically neutral. (The commitment is serious if we presuppose that mythology is true, phoney if we do not.)

The preceding considerations have some interesting consequences. One concerns mathematical notation whenever existence statements are made. For example, the postulate of group theory asserting the existence of inverses is: For all x in G there is a y in G such that $x \circ y = e$ (where e is the unit or identity element of G). This axiom is usually written in the form

$$(\forall_G x)(\exists_G y)(x \circ y = e). \tag{3}$$

But, if our interpretation of $\exists$ is correct, the axiom should be rewritten as

$$(\forall_G x)\,(\exists_G y)E_G y\,(x \circ y = e). \tag{4}$$

All the other existence statements should be symbolized in a similar fashion.

A second consequence of our deontologization of the "existential" quantifier is that it disposes of Quine's objection to second order logic (which quantifies predicates) for its alleged "excessive ontological commitment".

A third consequence of our consideration is far more substantial and of a technical nature. The usual existential interpretation of $\exists$ is responsible for the abandonment of an important intuitive principle of ancient and medieval logic. This is the principle of *subalternation*, according to which what holds for all holds for some. (More precisely, "All *F*s are *G*s" entails "Some *F*s are *G*s".) This principle does not hold if "some" is replaced with "there exist". Thus "All ghosts are playful" does not entail that *there are* playful ghosts. But it does entail that *some* ghosts are playful – an assertion that, like its premise, has no existential import. If the existential interpretation of $\exists$ is avoided, the principle of subalternation can be allowed to stay.

Another technical consequence of our reinterpretation of $\exists$ is that the axiom "$Fa \vdash (\exists x)Fx$" stays too, but reinterpreted as "If an individual has a certain property, then *some* individual(s) have the same property". If we wish to conclude existence then we must assume it to begin with, reformulating the preceding principle as follows:

For any inhabited context C, $Fa \,\&\, E_C a \vdash (\exists x)\,(E_C x \,\&\, Fx)$.

Finally, our reinterpretation of $\exists$ has an important philosophical consequence concerning the nature of elementary logic. We can now confidently assert, *pace* Quine, that *elementary logic is ontologically neutral rather than committed*. The deceiving appearance of ontological commitment comes from the mistaken reading of $\exists$ as "there exists" instead of "some". Existence is not just a matter of quantifiers but of either postulation or proof. (For additional arguments in favor of the ontological neutrality of logic see Bunge 1974c.) So much for "some" and its confusion with "there is".

A related problem is that of *analyticity*. An analytic formula is one which is true by virtue of its form or of the meaning of the concepts it contains. In the first case we call it a tautology, in the second a tautonymy. Examples: "All bachelors are bachelors" and "All bachelors are unmarried men" respectively. Quine (1952) challenged the analytic/synthetic dichotomy on the ground that we have no satisfactory concept of meaning, hence of synonymy, hence of tautonymy. But we claim to have supplied such a theory in Volumes 1 and 2 of our *Treatise*. Hence we keep the analytic/synthetic distinction although it is far less important than the formal/factual one. But we do admit that analyticity is *contextual* for both tautologies and tautonymies. The former is clear given the multiplicity of logics. Thus the ex-

cluded middle principle is tautologous in classical logic but not in intuitionistic logic. As for tautonymies, consider the following example. In the context of ordinary knowledge "Anything that bounces is solid and resilient" is tautonymous by virtue of the meaning of "bounces". Not so in the context of atomic physics, where elementary "particles" can bounce off atomic nuclei. Moral: To be correct, logical analysis must take propositions in context; grammatical analysis is parallel.

Let us now say a few words about *model theory*. But first let us repeat the warning issued in Vol. 5, Ch. 9, Sect. 1.2 concerning the ambiguity of the term 'model'. In factual science and technology a model is a specific theory supposed to represent (poorly or accurately) natural, social or artificial things of some kind. Thus one speaks of a mathematical model of a molecule, or a neural system, or an irrigation network. We call this the *epistemological* concept of a model.

In logic, on the other hand, a model is an example or "realization" of an abstract system, and a theory of such a model is an interpretation of the corresponding abstract theory. For example, there are arithmetical and geometrical models (examples) of abstract sets. And, in turn, abstract sets (along with the integers, matrices, and other constructs) are models of a ring, i.e. they satisfy all the formulas of ring theory. The key word here is 'satisfaction': a model of an abstract system ("structure") *satisfies exactly* all the conditions that define the system. The counterpart of 'satisfaction' in the case of scientific and technological models is 'representation': these models *represent approximately* their referents. A model of a lattice is another lattice: both are conceptual systems. On the other hand a mathematical model of a material entity is not another chunk of matter but a specific theory. The failure to distinguish the logical from the epistemological concepts of model (as in Sneed 1971 and Stegmüller 1976) has given rise to an utterly artificial philosophy of science. (See Truesdell 1984 for proof that this philosophy is remote from real science.)

Model theory deals exclusively with models in the logical sense, and has hardly anything to say about models in the epistemological sense. Two characteristic problems in model theory are to determine the relation between an abstract system and its models, and to characterize the relation between the latter. The reasons that model theory is nowadays included in logic are that (*a*) it has succeeded in elucidating one of the two notions of logical consequence, namely that of semantic entailment ($\models$), and (*b*) it has shown that an abstract theory can be proved consistent by exhibiting a model of it, i.e. by translating it into a "concrete" (but still purely conceptual) theory.

Model theory is of interest to philosophy for a number of reasons. Firstly, because it constitutes a natural enlargement of elementary (first order) logic. Secondly, because it exhibits the limitations of the purely syntactic approach to logic (which defines "⊢" but not "⊨"); and, with it, the impossibility of nominalism, which discards meaning and truth. Thirdly, because model theory highlights the difference between two kinds of mathematical system and, correspondingly, of mathematical theory: abstract (or "formal") and "concrete" (or specific). Thus an abstract group is one the nature of the elements of which is not specified beyond the axioms of general group theory. On the other hand the group of all rotations of space about a fixed point is a model of a group – hence another group – and it can be studied independently in geometry. Such rotations exemplify ("realize", as it used to be said) the abstract concept of group operation, and the theory of such rotations is a theory of that model of a group. (This is of course the gist of Klein's Erlangen Program: to ferret out the group-theoretic structure underlying every geometry.) An alternative model or example of a group is the set of integers with addition as the group operation, subtraction as the inverse operation, and zero as the unit element. Abstract theories are presumably the kind of mathematics Russell (1901) had in mind when he wrote that "mathematics may be defined as the subject in which we never know what we are talking about, nor whether what we are saying is true". This formalist description of mathematics fits "formal" (i.e. abstract) theories, not their models.

There are further reasons for philosophers to become interested in model theory. One is that it has contributed to elucidating the notion of formal truth in contradistinction to that of factual truth. Indeed, the notion of satisfaction of an abstract formula in a model exactifies the coherence theory of truth, according to which (formal) truth consists in the matching of conceptual systems. (Recall Vol. 2, Ch. 8, Sect. 2.1, and note that this notion of truth is radically different from that of adequacy.) A second reason is that model theory has clarified the notion of translation or interpretation of one theory into another – as when the formulas of group theory are translated into number-theoretic or geometric formulas. A third reason for a philosopher to become interested in model theory has to do with the implicit definition of the basic (primitive) concepts of a theory. Since any consistent abstract theory has an unlimited number of models, the postulates of the theory can define only the "essential meaning" of its primitives. (Equivalently: the "essence" of a family of models is the underlying abstract axiomatic foundation, which sketches the structure common to all models.)

A fourth reason is that, by giving birth to non-standard analysis (a generalization of the infinitesimal calculus), model theory has proved that logic *lato sensu* is not only an analytic tool but also a constructive one, and in any case anything but an idle formal game. And a fifth reason is that model theory has contributed to making mathematics more systemic or cohesive, by showing that certain previously disconnected theories are nothing but different "realizations" (interpretations or translations) of one and the same abstract theory – or, to indulge in metaphor, different embodiments of a single structure.

It is now the turn of the most famous (and misunderstood) of all metamathematical results, namely *Gödel's two incompleteness theorems*. The first asserts, roughly, the existence of some undecidable formulas in every consistent theory containing arithmetic – e.g. in arithmetic itself. (Formula A is said to be *undecidable* in theory T if neither A nor $\neg A$ is a theorem of T.) The second theorem states that no theory of that type can be proved consistent with the sole resources of the theory itself. (See e.g. Stoll 1963.) The first incompleteness theorem entails that formalization does not capture all our intuitions about natural numbers; the second, that mathematics as a whole cannot be proved to be consistent. (See Wang, 1974 for these and other implications.)

These results have made no impact on the practice of the working mathematicians, who carry on business as usual: they only "still produce nightmares among the infirm" (Smorynski 1977 p. 825). On the other hand they shattered Hilbert's noble program of proving by finitist methods the consistency of all mathematical theories. Consequently they had a profound impact on the foundations of mathematics, for which the year 1931 has been a watershed. Among the many remarkable results that followed was this: only a few theories are decidable (Tarski *et al.* 1968).

The incompleteness theorem made a strong impression on philosophers, particularly when misunderstood. The irrationalists read them as proof that reason had finally reached the end of its tether. And they likened the theorems to Heisenberg's "uncertainty relations", also misunderstood as setting a limit on human knowledge. Of course, mathematicians did not take a pessimistic view of the scope of human reason. If anything they admired Gödel for giving proof of a superior intellect: for suspecting, and then proving, totally unexpected theorems. And mathematics has made more advances in the half century that followed 1931 than in any other half century of its long history.

To a mathematician, Gödel's incompleteness theorems show only the

limitations of axiomatics and, in particular, of formalization (a particularly rigorous and exhaustive form of axiomatics, where all the cards are put on the table). The theorems boil down to the assertion that, given a body K of mathematical knowledge including arithmetic, and a formal system F built with the aim of formalizing K, there will always be formulas in K that are neither provable nor refutable in F. This technical result is hardly of philosophical interest: it only reminds us, once again, that there are few perfect human creations.

What does have some interest is Gödel's claim (in Wang 1974 p. 9) that his theorems confirm his own Platonistic philosophy of mathematics, according to which the natural numbers exist objectively just like the stars. This claim would hold only if there were some *evidence* for the existence of Plato's Realm of Ideas. In this case one could place the informal bodies of mathematical knowledge (the Ks of the preceding paragraph) in that ideal realm, and would regard their formalization (the Fs of a while ago) as poor man-made copies of those allegedly self-existing eternal verities.

But there is no such evidence. On the contrary, the history of mathematics teaches that every body of mathematical knowledge has been created by some living brain. Resorting to Plato's Realm of Ideas in themselves is like invoking divine providence every time we experience a fact which we are unable to explain. Moreover, a philosophy about the nature of constructs cannot be self-contained but must be part and parcel of a comprehensive ontology. It must account not only for the peculiarities of the fictions which we decide to endow with (formal) existence, but also for the human activity that creates such fictions; i.e. that ontology must include a philosophy of mind. Obviously, mathematical Platonism falls short of these requirements and, in particular, it is inconsistent with the scientific view that there are no brain-free ideas. We shall come back to this matter in Sect. 5.2.

The next item in our agenda is the *axiom of choice*, which has become a bone of contention of such magnitude that it has divided the mathematical profession into those who use it, or at least do not object to its use, and those who reject it. Yet the axiom looks innocent enough: For every family F of pairwise disjoint nonempty sets, there exists a set C containing exactly one element in common with every element of F. (See Figure 1.2.) The main reason, or unreason, for the controversy is that the axiom is nonconstructive: it asserts the existence of an object, namely C, without showing how to construct it, i.e. without specifying the choice function that maps F into C. (Hence, except for their heterogeneous origin the elements of the set C are quite faceless, ergo indistinguishable.) Being nonconstructive, the axiom

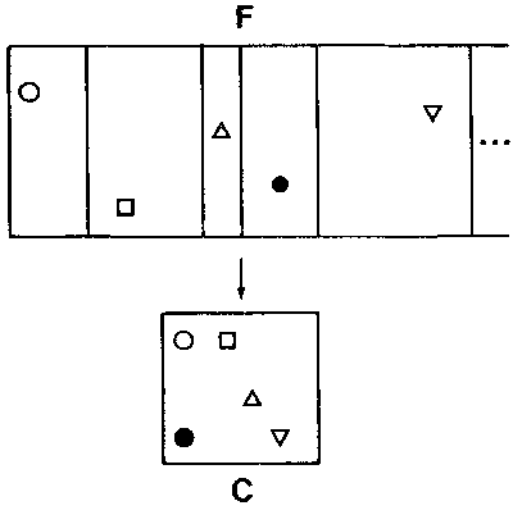


Fig. 1.2. The axiom of choice. Given an arbitrary partition of a set F into pairwise disjoint subsets, there exists a set C containing one representative of each of the subsets of F. No rule is given for forming C out of F

of choice cannot be accepted by the constructivists, who abide by the maxim that only that exists which can be effectively constructed. (See Sierpiński 1958 Ch. VI for a large collection of interesting opinions on this matter, and Moore 1982 for an exhaustive history.)

For better or for worse, the axiom of choice is the only nonconstructive postulate in classical set theory: all the other postulates can be interpreted pragmatically as instructions for forming new sets from given sets. Moreover, it has been proved (1963) that, though consistent with the remaining axioms of set theory, the axiom of choice is independent of them. This makes it possible to build much of mathematics with the help of a set theory not including the axiom of choice.

On top of being nonconstructive, the axiom of choice gives rise to some "paradoxes", though not to contradictions. The best known of them is perhaps the Banach-Tarski paradox which states that, using the axiom of choice, one can cut a ball into a number of pieces that can be put together so as to obtain two balls of the same size as the original ball. (See Jech 1973.) However, as Fraenkel (1966) has stated, dispensing with the axiom leads to even stranger theorems. And, above all, it results in a considerable impoverishment of mathematics, for the axiom appears in dozens of dif-

ferent though equivalent guises and disguises – so much so that it has been used repeatedly by some of its critics (Moore 1982). After all, intuitability is a subjective property and therefore not a reliable indicator of truth or validity. We shall come back to this point in Sect. 5.1: for the time being suffice it to stress that the controversy over the axiom of choice, still raging since its inception at the beginning of our century, shows that mathematics is not free from uncertainty and is not dominated by consensus over fundamentals. We shall come back to these matters in Sect. 5.1.

Our next question is whether set theory is necessary and sufficient to build the rest of mathematics. Until recently all mathematicians, except the intuitionists, answered this question in the affirmative, and nearly all philosophers followed suit. (See however a warning note in Bunge 1962a.) Indeed, all of "classical" (non-intuitionistic) mathematics seemed to be built with the sole help of set theory and its underlying logic, namely the ordinary predicate calculus. The Bourbaki treatise culminated the process of reduction of all mathematical concepts to those of set and membership. It is therefore understandable that the attention of most workers in the foundations and philosophy of mathematics should have focused on set theory, generally regarded as the bedrock of mathematics.

This situation was altered radically in the 1960s by two unexpected events. One of them was the proof that there is no such thing as *the* theory of sets: instead, there is whole family of different set theories. (A while ago we saw one source of multiplicity: the possibility of accepting or rejecting the axiom of choice. There are other axioms, in particular the continuum hypothesis, that are also independent of the remaining axioms for sets, so they can be kept or dropped.) In any event a new question of method or policy seemed to emerge: When stating that the whole of mathematics is reducible to set theory, which set theory is meant?

The second event that changed the foundational landscape in the 1960s was the discovery (Lawvere 1966) that the concepts of set and membership, which are basic (undefined, primitive) in set theory, are definable in another theory, namely *category theory*, founded in 1945. (See MacLane, 1971.) The basic notions of this theory are those of morphism – an extension of the concept of a function – and morphism composition. This being so, one must own that the question at the end of the last paragraph was ill-conceived, for it presupposes that some set theory has got to be the right and ultimate foundation of mathematics. This presupposition has been shown mistaken: sets have turned out to constitute just a model of a category; i.e. categories are more abstract, fundamental and inclusive mathematical objects than sets.

Category theory deserves to be closely examined by the philosopher of mathematics for the following reasons. First, it is currently the most basic theory on top of first order logic: it is the contemporary foundation of mathematics – mark the qualifier 'contemporary'. (See Figure 1.3.) Second, unlike all the other branches of mathematics, category theory deals wholesale with mathematical systems ("structures"), such as the totality of topological spaces. (These totalities are categories, not sets. And the way to deal with such totalities is by means of morphisms, some of which are structure-preserving whereas others are not. For example, a group morphism relates groups, whereas a certain "forgetful functor" relates groups to semigroups by "forgetting" the inversion operation.) Third and consequently, category theory has not only enriched mathematics but has also exhibited a previously hidden network binding together the various mathematical systems ("structures") and consequently their corresponding theories. It is just as universal and cross-disciplinary as first order logic. Thus category theory is accomplishing one of the ideals of the founders of set theory, namely the unification of mathematics on the basis of the most general concepts.

After having sampled the problematics of the philosophy of logic, we are ready to face the last question in our agenda: *What is logic*? (See Kneale and Kneale 1962 for the history of this question, and Hacking 1979 for some recent developments.) An enumeration of the disciplines that are currently counted as logic, such as standard first order logic, model theory, set theory, and proof theory, does not constitute a satisfactory definition because there are numerous non-standard logics, and because all branches of logic are likely to keep growing. We must know what is constant throughout such

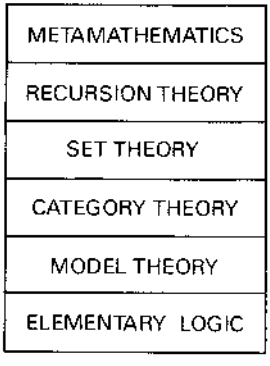


Fig. 1.3. The basement of the mathematical building as of 1985.

changes. The safest answer seems to be this: Logic *stricto sensu* is the basic theory of deduction (a theory completed with model theory), and logic *lato sensu* is the family of all the more general mathematical theories.

(Logic *stricto sensu*, or first order predicate calculus, holds of course a particularly important place in mathematics. Firstly, it has been proved to be both consistent and complete. Secondly, and more important, the predicate calculus is distinguished from all the other mathematical theories in that it presupposes nothing. In fact, once the formation and inference rules have been laid down, everything else follows: elementary logic is its own foundation.)

Being the family of all the most general mathematical theories, logic constitutes both the *foundation* and the *language* of the rest of mathematics. It is the foundation in the sense that every mathematical theory presupposes some branch of logic. (This view has of course a logicist pedigree. Intuitionists regard logic as an offshoot of mathematics, and formalists hold that mathematics presupposes an even more basic theory: that of strings formed out of a finite number of characters.) That this is actually so is shown by axiomatizing mathematical theories in the exhaustive fashion taught us by Tarski (1956). This requires mentioning explicitly the fragment of logic underlying the given theory.

Logic doubles as the basic language of the whole of mathematics, in the sense that every mathematical formula is a particular case of a formula of logic *lato sensu*. (Even Dummett, 1977, who follows Brouwer in holding logic to be derivative, uses the language of logic to write down principles of intuitionistic mathematics.) Thus "$x < y$" is an instance of "Rxy", "d sin x = cos x dx" exemplifies "$Fx = Gx$", and "x is a natural number" is a particular case of "$x \in \mathbb{N}$". Caution: The use of elementary logic as a language does not prove that logic is *nothing but* a language. Indeed, whereas languages make no truth claims, theories do make such claims, and logic happens to be a family of theories. Thus the arithmetic falsity "1 = 2" instantiates a formula of the predicate logic with identity: the latter is incompetent to assign it a truth value. In sum, logic is the universal language of mathematics (in addition to being the theory of its deductive structure), in the same sense that mathematics is the universal language of science and technology.

The following subsection will illuminate further facets of the question of the nature of logic.

3.2. *Non-Standard Logics*

The logic *stricto sensu* underlying practically all mathematical theories is classical logic, the core of which is the standard predicate calculus with identity. (The two main extensions of this theory, the second order predicate calculus and infinitary logic, occur as well though not as conspicuously.) This is not to say that all mathematicians make explicit use of classical logic. In fact most don't and some, such as Poincaré, Brouwer and the Bourbaki group, have been hostile to logic. Just as most birds probably do not realize that they fly in air, so some mathematicians may not realize that every one of their formulas is an example of a well formed formula of logic, and that every one of their valid reasonings fits into some of the inference patterns canonized by logic.

In addition to classical or ordinary logic there are hundreds of alternative or non-standard systems of logic. (See Gabbay and Guenthner, Eds. 1983–85, Vols. II and III) Among them we recall intuitionistic logic – the only non-standard logic most mathematicians have ever heard about – many-valued logics, modal logics, relevance logics, free logics, quantum logics, and paraconsistent logics. Unlike mathematicians, philosophers of an exact cast of mind are often fascinated by non-standard logics, as can be seen by perusing the *Journal of Philosophical Logic* and the newer *Journal of Non-Classical Logic*. This is not surprising, because most of the non-standard logics have had philosophical motivations, and all of them pose interesting philosophical problems. Suffice it to mention the following: What makes a logical system non-standard?, What are the motivations for the various non-standard logics?, What can one do with them, i.e. what peculiar problems do they help solve?, Are all logics equally worthy, or is there one which is incontestably superior to all others, and What becomes of the unity of science, and even rationality, if there is a plurality of canons?

Before tackling these problems it will be convenient to stress that we shall be concerned mainly with *formal* logics, i.e. theories of deductive inference lacking in ontological, epistemological, or ethical content and presuppositions. This warning is necessary because there are numerous theories which, though called 'logics', are not strictly such because their distinctive basic (primitive) concepts are nonlogical. For example, inductive, epistemic, doxastic, and erotetic "logics" belong in epistemology. In fact, the central concept of the first is that of inductive support, that of the second is certain (as opposed to conjectural) knowledge, that of the third is opinion (or belief), and that of the fourth is problem (or question). Likewise tense logic

includes the ontological concept of time, preference logic the psychological and value-theoretic of preference, and deontic logic the ethical notion of duty (or obligation).

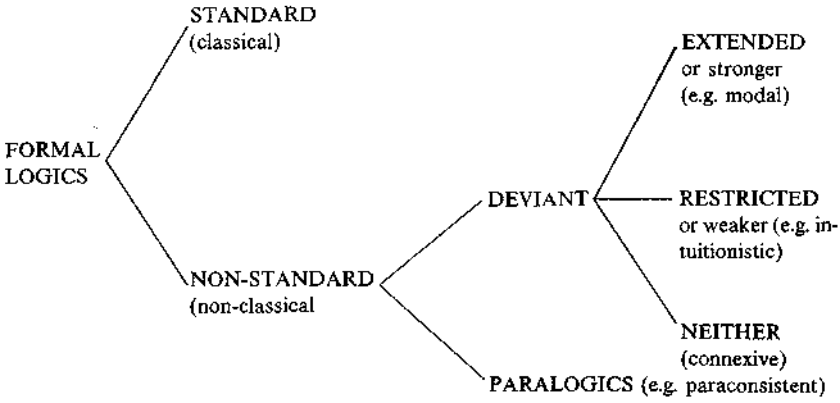
We shall hardly be concerned with these "logics" because they are not purely formal and, far from aiming at elucidating the relation of logical consequence, they all presuppose (classical) formal logic. Besides, we found no use for inductive, epistemic and doxastic logic in Vols. 5 and 6. Nor does one need tense logic in science or in technology, where time – like place – is one among the many "variables" that ordinary logic can handle. (For example, "x happened at t" can be formalized tenselessly as "Hxt & $t <$ present time".) As for the "logics" of preference and duty, as well as those of choice and decision, we shall look into them in Vol. 8. In conclusion, we shall focus our attention on formal logics or, rather, on some of the philosophical problems they raise.

It will prove convenient to start by classing the large, growing, and rather unruly family of formal logics. Our first partition of this family will be the standard/non-standard dichotomy. A *non-standard logic* is any system of formal logic that either fails to contain some classical "principles" – such as those of contradiction, excluded middle, bivalence, or distributivity – or contains additional laws (axioms or theorems). More on this in a moment.

In turn we divide the non-standard logics into those which which do, and those which do not, include the principle of contradiction "$\neg(p \,\&\, \neg p)$". We call *deviant logics* the former and *paralogics* the latter. And we divide the collection of deviant logics into those which extend classical logic, those which restrict it, and those which are neither contained in nor contain it. (See different classifications in Haack 1978 and da Costa 1982a.)

The collection of *extended* deviant logics, i.e. of systems that include classical logic, is exemplified by any classical modal logic, which contains an additional primitive concept, namely that of possibility. The family of *restricted* deviant logics is by far the more numerous: it includes Aristotle's syllogistics, intuitionistic, quantum, relevance, free, and negation-less logics. Classical logic is a supersystem of every one of them, i.e. it is retrieved by enriching the deviant system with one or more principles. The deviant logics that are neither extended nor restricted are a few curiosities such as the *connexive* logics. (See Anderson and Belnap 1975 pp. 434–452.) Finally the *paralogics*, which do not abide by the principle of contradiction, include paraconsistent, universal, nonsense, and fuzzy logics.

In short, we class formal logics as follows:



So much for the bewildering variety of non-standard logics. Let us now deal with the first question in our initial list, namely *What is a non-standard logic*? or, put in methodological or pragmatic terms, *How can we recognize logical heterodoxy*? A while ago we gave an apparently simple answer, to wit, a non-standard logic is one that differs from classical logic in at least one of the traditional principles (axioms or rules). The simplicity of this answer is, like all simplicity, deceiving, for there are several alternative formulations of classical logic, and they may not all be strictly equivalent. (I.e. some of them may not systematize exactly the same body of knowledge.) In other words, it is not at all sure that mere inspection of a set of postulates (axioms and rules) of a system of logic suffices to identify it as classical or as non-classical – unless of course one decides on a given classical system as the canonical one. (In this case Tarski's 1965 formalization of predicate logic would be an obvious candidate.)

If form or syntax is not an unambiguous indicator of logical orthodoxy (or heterodoxy), what about interpretation or semantics? Take the case of the "existential" quantifier. As we saw in Sect. 3.1, "$(\exists x)Fx$" can be assigned different interpretations, such as "There are Fs", "Some individuals are Fs", "Some substitution instances of Fx are true", "Individuals with property F can be found", and "An example of an F has actually been constructed". The latter interpretation is favored by the intuitionists, who interpret "p" as "We can confidently assert p", or "p is provable". In fact intuitionistic logic does not differ significantly from classical logic with

respect to form: the main difference resides in the interpretation. (However, that small syntactical difference in the underlying logic generates a momentous difference between intuitionistic and classical mathematics, if only because the former does not allow for proofs by *reductio ad absurdum*.) That difference in interpretation or semantics derives from a profound difference in the conception of the nature and role of logic. Whereas to the classical logician logic codifies valid inference, to the intuitionist logic is a means for attaining certain or infallible knowledge. So, in this case the difference is of a philosophical nature. Unfortunately not even a difference in interpretation suffices to identify unorthodoxy, especially when marginal, because – as recalled above – every classical formula can be interpreted in different ways.

How then can we recognize logical heterodoxy? Miró Quesada (1983) has proposed a suitable criterion, which is historical or pragmatic as well as comparative: A logical system is the more heterodox, the more removed it is from mainstream research (i.e. the more it diverges from the evolving tradition). This criterion serves for the synchronic comparison of logical theories but, of course, it does not apply to diachronic comparisons. (Current classical logic might have been regarded as non-standard by Aristotle and perhaps even by Frege.) By this criterion intuitionistic logic, which includes the principle of contradiction, and which underlies some mathematical research, is a mild breeze by comparison with the tornadoes of paralogics. The great majority of mathematicians and philosophers regard the principle of contradiction as *the* over-arching principle of logic, and are therefore inclined to regard paralogics (in the improbable case they have heard of them) not just as deviations from orthodoxy but as aberrations. They are not afraid of novelty but they cannot stand anarchy. We shall come back to this point.

Our second question is that of *motivation*. We all know that the initial motivation for the inception of mathematical logic in the mid-nineteenth century was to update and exactify logic by unearthing, working out, and systematizing the inference patterns actually employed by mathematicians, who had outstripped the ancient and medieval logicians. That motivation was then strictly mathematical; so was the outcome, namely the mathematization and enormous enrichment of classical logic. (Few if any philosophers took notice of this revolution in logic during half a century.) In contrast, the motivations of most non-classical logicians have been philosophical. Let us take a look at a few typical cases.

By far the best known nonstandard logic is the intuitionistic system founded by Heyting. The motivation for it was to formalize and systematize

the inference patterns allowed by mathematical intuitionism, one of the main classical philosophies of mathematics. Hence intuitionistic logic inherited the philosophical theses of mathematical intuitionism. Let us recall a few of them. (For a more detailed critical examination see Bunge 1962a.)

Two basic philosophical tenets underlying intuitionistic logic are (*a*) the *pragmatist* maxim "To be is to make something or to be constructed", and (*b*) the *verificationist* (or *operationist*) doctrine of meaning: "The meaning of a statement is the way it is justified (e.g. proved)". These two philosophical views are the foundation of the principle of *constructivity*: "Every mathematical or logical statement expresses the result of a construction" (Heyting 1956a p. 223). Nonconstructive concepts, statements, and proofs, are consequently to be avoided if not outright rejected. Thus where the classical logician might rest satisfied with proving a disjunction, the intuitionist wants to make sure that at least one of the disjuncts be either self-evident or provable. This holds even when one of the disjuncts is the negate of the other: intuitionists neither assert nor deny the excluded middle principle: they just do not use it. Indeed they reject, quite rightly, the idealistic dogma that every proposition *is* true or false regardless of whether anyone has taken the trouble of checking it. And they do not care for existence axioms or theorems unless accompanied by algorithms or by examples. Nor, finally, do they admit indirect proofs – i.e. proofs by *reductio ad absurdum* – which are probably the most numerous (because the easiest) in logic and mathematics.

A quick way of getting a feeling for intuitionistic logic is to look at the manner in which it handles propositions and their denials. To an intuitionist the propositional symbol 'p' should be read 'p is either self-evident or provable', and '$\neg p$' should be interpreted as 'p is either counter-intuitive or undemonstrable'. He admits the law "$p \Rightarrow \neg\neg p$" because he interprets it in an intuitive methodological manner: "If p is demonstrable, then it is not demonstrable that p is not demonstrable". On the other hand he does not admit the converse, namely "$\neg\neg p \Rightarrow p$", because the fact that it is not demonstrable that p is not demonstrable implies nothing about p. None of this affects the classical logician, who feigns that p, $\neg p$, $\neg\neg p$, and the rest, are eternal objects independent of our ability to prove or disprove them. In particular, the classical logician does not feel compelled to construct p in order to assert p.

The restrictions imposed by the principle of constructivity have proved to be double-edged. On the one hand they have prompted the invention of a number of new constructs as well as of direct proofs, which have enriched

mathematics. On the other hand this enrichment has not been compensated for by the massacre of classical results cherished by the vast majority of pure and applied mathematicians. Among the uncounted results condemned by intuitionism, at some time or other, as either meaningless, uprovable, or false, are the following: (*a*) every real number is positive, negative, or zero; (*b*) if a and b are real numbers, and $ab = 0$, then either $a = 0$ or $b = 0$; (*c*) every set is either finite or infinite; (*d*) every function continuous in a closed interval has at least one maximum – an existence statement; and (*e*) another existence statement: if a continuous function has a negative and a positive value in an interval, then it has at least one zero in the same interval: see Figure 1.4. Paradoxically, the constructivist constraint has excised from mathematics a great number of intuitive results.

Another philosophical motivation of intuitionistic logic is, of course, the variety of infallibilism known as *intuitionism*: "A mathematical construction ought to be so immediate to the mind and its results so clear that it needs no foundation whatsoever" (Heyting 1956b p. 6). Accordingly one should choose as basic notions the most immediate and obvious ones, such as that of a natural number. A first objection is that intuitability and clarity are subjective or personal, so that they cannot be used as objective criteria for the choice of the basic (primitive) concepts of a theory. If we were to allow our personal intuition to judge the worth of logical or mathematical constructs, then most of mathematics (including intuitionistic mathematics)

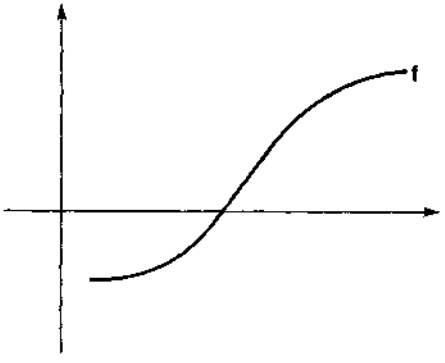


Fig. 1.4. The intermediate value theorem. This existence statement has no place in intuitionistic mathematics because it is not accompanied by a constructive procedure for determining the exact point at which the curve crosses the x-axis. Yet it is surely intuitive and, moreover, of daily use in science and technology.

would be condemned. Remember that mathematics and science start where ordinary experience and intuition leave off (Vol. 6, Ch. 14, Sect. 2). A second objection is that, like it or not, as a matter of fact most mathematical constructs do have some "foundation" or other, i.e. they presuppose others – e.g. because they are built out of more basic constructs. The only foundation-free branch of formal science is ordinary logic, whether classical or intuitionistic.

Finally, it is interesting to note that intuitionism is infallibilist with regard to mathematics but not with regard to logic. In fact according to Brouwer the laws of logic are neither a priori nor eternal: they are corrigible hypotheses rather than immutable regulative principles. The history of logic confirms this thesis up to a point, just as the history of mathematics refutes the intuitionist dogma that there are self-evident and absolutely secure mathematical principles. All mathematical statements and proofs ought to be viewed in principle as capable of being corrected, in particular turned more rigorous. Whether or not the principles of logic should be regarded in the same vein, shall be discussed below.

However, we should draw a distinction that escapes intuitionists, radical empiricists (in particular pragmatists), and vulgar materialists. This is the difference between the *history of the interactions* between logical and mathematical research, on the one hand, and the *logical* relation between logical and mathematical theories considered in themselves, on the other: see Figure 1.5. Logically, mathematics rests on – i.e. it presupposes – logic *lato sensu*. On the other hand the empirical relation between logic and mathematics, i.e. that between the respective research communities, is quite symmetrical. In fact, this relation is one of *mutual and progressive adjustment*.

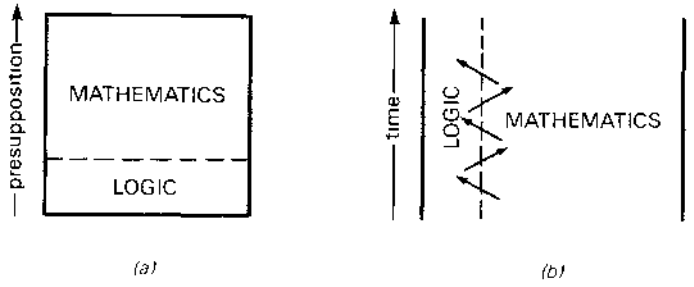


Fig. 1.5. The two relations between logic and mathematics: (*a*) the logical relation of presupposition, and (*b*) the epistemological (and social) relation of mutual inspiration and control.

The validity of mathematics consists in its abiding by the laws of logic, and the validity of logic rests upon its power to analyze and control mathematical research – or rather its deductive aspect. This is not a vicious circle but a virtuous one: it is a process of successive approximations (Bôcher, 1905).

In conclusion, the philosophical motivations of intuitionistic logic are, like those of intuitionistic mathematics, suspect. However, the product is far from worthless: intuitionistic logic is an interesting piece of mathematics. Moreover, properly interpreted – perhaps in epistemological or pragmatic terms, as originally intended – it may yet acquire philosophical significance in addition to showing that it is possible to reason rigorously in non-standard ways.

The next best known nonstandard logic is *many-valued logic* and, in particular, its three-valued version. It has had several motivations (Łukasiewicz 1970). One of them is semantical (or epistemological), namely the view that some propositions are neither true nor false, but just "possible" (or possibly but not actually true or false). In turn, this semantical thesis has a metaphysical substratum, namely indeterminism and, in particular, the doctrine of future contingents, which goes back to Aristotle. Consider the classical example: whether or not there will be a sea battle tomorrow is not fully determined by present conditions, and this quite apart from our inadequate knowledge of the latter. Consequently, the proposition "There will be a sea battle tomorrow" is just as "possible" as its contradictory, namely "There will not be a sea battle tomorrow". So, neither of them should be assigned the value "true". Three valued logic assigns them the truth value $\frac{1}{2}$, or else "indeterminate"; moreover, it introduces important changes in the axioms. (See Rescher 1969.)

In Vol. 2, Ch. 8, we chose to keep classical logic and give up instead the assumptions that every proposition has a truth value (whether we know it or not), and that the only possible truth values are 0 and 1. This strategy allows us (*a*) to abstain from assigning truth values to the above propositions about the future sea battle (or any other indeterminate events), (*b*) to handle half-truths, such as "This book is thoroughly wrong", by introducing the notion of graded or partial truth, and (*c*) to retain ordinary logic as the basis of mathematics, as well as the common core of all scientific theories, formal and factual. The latter is no small advantage compared to the staggering cost of rebuilding the whole of mathematics and science to conform to some system of many-valued logic. (The same holds, of course, for any other deviant logic.) But, again, this does not detract from the intrinsic interest of many-valued logic.

Our third example of a nonstandard logic is *modal logic* (Lewis and Langford 1959, Hughes and Cresswell 1968). C.I. Lewis had two motivations for initiating it. One was the wish to form a concept of logical consequence untainted by the so-called paradoxes of implication; the other was to elucidate the notions of necessity and possibility. The logical motivation can be defused by noting that mathematics need not recapture all of our premathematical intuitions. (Besides, it soon became apparent that modal logics themselves are ridden with paradoxes; e.g. the denial of a necessary proposition strongly implies any proposition.) As for the notion of logical consequence, it was elucidated several years later by model theory, which taught us that (*a*) entailment is stronger than implication (i.e., if p entails q, then p implies q, but not conversely), and (*b*) there are two different though related concepts of entailment: the syntactical ($\vdash$) and the semantical ($\models$) relations, both of which exactify the vague notion of logical (or necessary) consequence. (Necessarily, if p then q $=_{df}$ $p \vdash q$. Necessarily, if p is true, then q is true $=_{df}$ $p \models q$.)

What about the ontological motivation for modal logic, i.e. seeking to elucidate the notions of physical (or ontic) possibility and necessity? Unfortunately modal logic does not do this job either. As we saw in Vol. 3, Ch. 4, Sect. 2, in factual science a fact is regarded as being possible only if it fits some objective (e.g. natural) law – and such law need not be causal. Therefore a science-oriented ontology has no more use for modal logic than mathematics. Still, the failure of modal logic to elucidate the notions of entailment and of real possibility does not detract from its interest as a rich family of rigorous and ingenious mathematical systems. Moreover we cannot rule out the possibility that, properly interpreted, modal logic may still acquire some philosophical significance.

Next in our list comes *relevance logic* (Anderson and Belnap 1975). The motivation for this system is to avoid the admixture of heterogeneous universes of discourse permitted by classical logic (though prohibited by totle). We saw one example of this only one minute ago: a falsity implies anything and, in particular, *ex contradictoriis quodlibet*. Another, and far more frequent, source of irrelevance is the principle of addition: $\ulcorner p \vdash p \vee q \urcorner$, where q need not be related to p. Relevance logic attempts to reform classical logic so as to respect relevance, i.e. to stick to the original universe of discourse. However, it does not succeed, because it keeps a major offender, namely the principle of addition. And it does well to keep this principle, for we need it in open (e.g. developing) contexts, where we wish to retain the freedom to relate subject matters that, though at first blush

mutually foreign, may prove to be intimately connected. After all, relevance is contextual not absolute.

I submit that the very project of building a relevance logic is misguided because the concept of relevance is not only contextual but also semantical rather than syntactical. Relevance is a matter of reference, and relevance logic does not contain a theory of reference (for which see Vol. 1, Ch. 2). Relevance can be controlled by alternative means. For one thing, if we need to secure relevance (or homogeneity of the reference class), we can do so by axiomatizing the body of knowledge concerned. Indeed, axiomatization specifies in advance the universe of discourse and it guarantees reference conservation (Vol. 1, Ch. 2, Sect. 4.2.). Secondly, if (in an open context) we suddenly encounter a total stranger to our originally intended referents, then we may suspect either of two things: that the irrelevance is real, or that it is apparent. In the first case irrelevance will be a product of some contradiction that has gone unnoticed. (By the way, this shows the usefulness of the "paradox" *ex contradictoriis quodlibet*.) But the irrelevance may be only apparent: we may have discovered a previously unsuspected link between two reference classes and their corresponding research fields. Classical logic may help us in the first case, relevance logic in neither. (See further objections in Copeland 1980.)

Let us now peek at the philosophical motivations for the various "logics" of change. One of them is *temporal* (or *tense*) "logic" (Prior 1967, Rescher and Urquhart 1971), disqualified earlier as a logic proper because it contains the ontological concept of time. One such system of temporal logic is nothing but the classical predicate calculus enriched with an extralogical predicate interpreted as "is an instant of time". Another such system may be construed as the theory of partially ordered sets (which presupposes classical logic), in which the basic set is interpreted as the collection of (absolute) instants, whereas the order relation is interpreted as "(absolutely) prior to or simultaneous with". None of these theories conceives of time as relative to some reference frame or even as a continuous parameter; hence temporal "logic" is of no interest to scientific ontology. In sum, temporal "logic" is a miniontology of no interest to either philosophy or the foundations of science: it is but an academic exercise in exact philosophy.

(As for the linguistic aspect of the question, it should be noted that every tensed sentence can be transformed into a tenseless one by an appropriate use of the concept of time – paradoxical though this may seem. Thus "*a* came before *b*" can be converted into "*a* precedes *b* in time", or "*a* is prior to *b*" – perhaps with the addition of the clause "relative to frame *c*". These

tricks do not expel becoming from language: they just deprive the present, which is egocentric, from its privileged position. Hence they allow us to treat all instants of time, relative to a given reference frame, on the same footing. This results in greater objectivity and universality – which is why it is the method adopted in science and technology.)

However, the most radical of the "logics" of change is not a miniontology, like temporal "logic", but *paraconsistent logic* (da Costa 1980, Arruda 1980, Routley 1980). Its peculiarity is that the principle of contradiction is not a logically valid schema in it, whence it would be the "logic" underlying inconsistent theories, such as naive set theory. On the other hand most other logical principles, including the excluded middle and the modus ponens, are valid in paraconsistent logic – which is itself consistent.

One motivation of paraconsistent logic is the wish to avoid the classical law *ex contradictoriis quodlibet* (a contradiction entails anything). This permissiveness is embarrassing because (*a*) it condones *trivial* systems, i.e. systems all the formulas of which are theorems for being entailed by a single contradiction; and (*b*) it makes it hard to *evaluate* axiom systems by checking only their consequences. However, there are two ways of avoiding that law without giving up rationality. One is to prove the consistency of the axiom system of interest or, even better, to compress all of its axioms into one. (This is one of the reasons for preferring physical theories based on a single variational principle.) Another is to adopt intuitionistic logic, for it demands that the antecedent of every conditional be provable. Either procedure, though expensive, keeps us in business, whereas adopting paraconsistent logic amounts to declaring bankruptcy. Besides, as we saw a moment ago, the law *ex contradictoriis quodlibet* is useful in open contexts: it helps us suspect irrelevance from contradiction and conversely. In sum, there is no logical justification for adopting paraconsistent logic.

Another motivation for paraconsistent logic is ontological, namely the conflation of logic with ontology and the wish to accomodate dialectics, according to which change is contradictory, so that some contradictions would be true of our changing real world (da Costa 1980, 1982b, Routley 1980). In particular, ultralogic – the most ambitious of all paraconsistent logics – would be about everything thinkable, whether conceptual or material, possible like a black hole or impossible like a round circle: "It provides a canon for reasoning in every situation, including illogical, inconsistent and paradoxical ones" (Routley 1980 p. 893). Thus the classical rationalistic illusion that reason is self-sufficient ends up by destroying rationality.

We have argued before (Sects. 1 and 2) that logic and pure mathematics

are only about constructs, not material things. Consequently a universal logic or ontology *à la* Meinong, dealing with anything thinkable, must be either utterly trivial or false: constructs and concrete things have no laws in common. (See also Vol. 3, Introduction, Sect. 2.) We have also argued (Vols. 5 and 6) that the study of the real world involves empirical procedures: that no purely a priori theory can be hoped to be true of concrete things. Hence in our perspective any attempt to build an a priori theory encompassing objects as heterogeneous as inferences and quantum jumps is wrong-headed.

As for dialectics, we have argued elsewhere (Bunge 1975a) that it is not an adequate theory of change and novelty – except insofar as it stresses the centrality of both – if only because it regards change as contradictory. (This view seems to have originated in the Eleatic analysis of motion. The arrow was said *to be and not to be* at a given place at a given time. Science analyzes motion, and change in general, in a totally different fashion, namely by *blending* the concepts of thing (or being) and change (or becoming) rather than opposing them. Thus we say that the arrow *moves* with velocity v when passing through place x at time t. We assume, more generally, that every thing is at a given time, relative to some reference frame, in some state (which may be composite). And in turn a state may be construed as a point or stage in some process or other: Vol. 3, Ch. 5). True, some processes are mainly conflictive or dialectical; but others are mainly synergic or cooperative, and still others are neither or are a combination of several basic modes of change. In any event change (*a*) has nothing to do with logical contradiction, and it is not within the domain of the logician, and (*b*) it is representable by factual theories which have classical (at most intuitionistic) logic built into them. In short, there is no legitimate ontological or scientific rationale for doing paraconsistent logic. On the other hand there is a powerful reason for avoiding it, namely the defense of rationality.

Still, the nagging question remains: *Why* should rationality involve the principle of non-contradiction? Certainly not because contradictions are unthinkable: we often incur them. Nor because they are unintelligible: we all understand a contradiction such as "The world is small and big". Nor because they are meaningless: in fact contradictions are just as meaningful as tautologies. (Our result on the meaning of a tautology, in Vol. 2, Ch. 7, Sect. 2.1, holds also for its negation.) Nor, finally, because contradictions are false: it would be up to us to decree them true. I submit that we value non-contradictoriness for extralogical reasons. Firstly because we cherish truth, which in turn involves consistency: think of the prospects of finding

out the truth about the author of a crime on the sole strength of the accounts of two witnesses, one of whom swears that *A* did it, and the other that *A* did not do it. Secondly because, when interpreted in practical terms, contradiction leads to inaction: indeed it is impossible to do *A* while at the same time not doing *A*. In conclusion, we value consistency mainly for practical reasons. By the same token dialectics, far from being "the algebra of revolution" (Lenin), condemns us to inaction and confusion. So much for paraconsistent "logic" and its motivations.

Another system that we have characterized as paralogical is *fuzzy logic* (Zadeh 1975). Its goal is to describe ordinary reasoning, which is for the most part imprecise because it contains fuzzy predicates, such as "long", and plausible (e.g. analogical) inferences rather than valid ones. Indeed, far from being normative like classical logic and all of the deviant logics proper, fuzzy logic "aims [...] at an accomodation with the pervasive imprecision of human thinking and cognition" (Bellman and Zadeh 1977 p. 106). This is of course a fascinating problem for cognitive psychology and epistemology (Vol. 5, Chs. 5 and 6). However, it is misleading to call that study 'logic' if the whole point of it is to *legitimate* fuzzy notions and invalid inferences instead of discovering and correcting them. (On the other hand fuzzy "logic" is a priori in so far as it makes no use of empirical procedures for finding out how people actually reason.)

Fuzzy logic is defeatist: it "represents a retreat from what may well be an unrealizable objective, namely the construction of a rigorous mathematical foundation for human reasoning and rational behavior" (Zadeh 1975 p. 426). The historical evidence points to the opposite course: it points to the advancement of rigor – though not without some temporary setbacks. And even if it did not, it is the duty of the logician to reduce fuzziness and invalidity rather than to portray them or shove them under an attractive formal rug. What is the point of formalization unless it is to substitute precision for fuzziness, and validity for sloppy inference? What is the function of the torch but to help us find our way in the dark?

Finally, let us examine the motivations for *quantum logic*. But first we must dispel a common confusion, that between quantum *logics* and the peculiar *algebras* used in quantum theory. This confusion stems from Birkhoff and von Neumann (1936), who conflated the concept of proposition with that of a projection operator, only because both have two possible values, namely 0 and 1. This confusion is facilitated by adopting an "operational" redefinition of a proposition that is likely to elicit a smile from any logician. One such redefinition, originally suggested by the physicist Jauch,

is this: "Propositions are observables the measurement of which can only give one of two values conventionally called *yes* and *no*. Hence, a statement is a proposition only if an instrument, by means of which its content can be verified, can be conceived. Conventional quantum mechanics associates projectors to propositions" (d'Espagnat 1973 p. 717, also 1975). We shall steer clear from such confusion and shall deal exclusively with quantum logics proper, i.e. the systems of nonstandard logic that have sometimes been claimed to underly quantum theory.

Two main motivations have been given for devising quantum logics. One is the fact that quantum theory contains mutually incompatible (or "complementary") propositions, and therefore propositions that cannot be truly asserted jointly. The other is the claim that certain experiments, notably those involving the diffraction of electrons and the like, refute the distributive law for propositions. (See e.g. Friedman and Putnam 1978.) To understand the first motivation let us recall that quantum mechanics represents dynamical variables by operators, and that some of these do not commute with one another: i.e. if $\hat{A}$ and $\hat{B}$ are such operators, it may be the case that $\hat{A}\hat{B} \neq \hat{B}\hat{A}$. In this case the proposition "The value of A is a" is incompatible with (or complementary to) "The value of B is b". (For example, "The value of the x-component of the angular momentum is $m\hbar$" and "The value of the y-component of the angular momentum is $m\hbar$" are not both true of a given thing in a given state at a given moment.) The notion of complementarity may be regarded as a generalization of that of contradiction – provided one does not forget that the former is contingent whereas the latter is logical. Indeed that two propositions are mutually complementary is not a matter of form but of matter: it represents a feature of reality. If A and B fail to *have* simultaneous sharp values, then we cannot truly attribute them such values. On the other hand we can attribute them simultaneous distributions of values. And the more peaked the distribution of A, the less so that of B, and conversely.

Now, it would make no sense to build a system of logic that excludes mutually complementary propositions, as quantum logic is sometimes said to do. Logic must be comprehensive enough to allow us to state mutually complementary (and even contradictory) propositions. It is up to empirical research to find out which if any of the components of such complementary propositions is true. This holds not only for quantum theory but for every research field and every application of logic. It happens even with reference to daily life. Thus the propositions "She is swimming now" and "She is typing now" can never be both true for the same *she* and the same *now*, but

this is a material constraint that does not affect logic. All we have to do when encountering mutually complementary propositions is to abstain from stating them jointly. This, rather than stating that the underlying logic is non-Boolean, is the reasonable attitude to take. To do otherwise is to confuse logic with ontology or even physics.

As for the claim that quantum mechanics violates the distributive law for propositions, let us illustrate it with the help of an example used above. Consider the propositions

A = ⌜The x-component of the angular momentum of the electron is $m\hbar$⌝

B = ⌜The y-component of the angular momentum of the electron is $m\hbar$⌝

$C = \neg B$,

where the electron referred to in all three propositions is a given electron in a given state at a given moment. Now, the distributive law, for propositions is the equivalence

$$A \,\&\, (B \vee C) \Leftrightarrow (A \,\&\, B) \vee (A \,\&\, C).$$

Since $B \vee C$ is a tautology in ordinary logic, the *LHS* of the equivalence amounts to A, so it is true if A is. Consider now the *RHS* of the distributive law. In our example $A \,\&\, B$ is false because A and B are mutually complementary propositions. (The corresponding operators do not commute and therefore have no simultaneous sharp values. This, in our realistic interpretation, is why we cannot measure such sharp values on one and the same object at the same time: see Ch. 2, Sect. 4.1.) On the other hand $A \,\&\, C$ is true if A is, for when the x-component of the angular momentum has a sharp value, the y-component does not. Therefore the *RHS* is true if A is. But, unlike the *LHS*, the *RHS* does not simplify to A. Hence it would seem that, indeed, the distributive law fails in quantum mechanics.

Worse, we need not resort to quantum physics to find apparent cases of non-distributivity. Take the case

A = ⌜She is swimming now⌝

B = ⌜She is typing now⌝

$C = \neg B$.

Here too the *LHS* of the distributivity law simplifies to A, whereas the *RHS* would seem not to. So, the quantum has nothing to do with the apparent failure of distributivity. The latter has to do with an incomplete analysis of the argument.

Indeed, in the above two arguments we have made tacit use of the premise that A and B are mutually "complementary" (incompatible), i.e. that

$\neg(A \,\&\, B)$. We employed this premise in evaluating the first disjunct of the *RHS* of the distributive law. If this extra premise is taken into account, it turns out that distributivity holds after all. The proof runs as follows

LHS	*RHS*
1. $A \,\&\, (B \vee \neg C)$ assumption	1′. $(A \,\&\, B) \vee (A \,\&\, \neg B)$ assumption
2. A 1 and simplification	2′. $\neg(A \,\&\, B)$ tacit assumption
3. $\neg(A \,\&\, B)$ tacit assumption	3′. $A \,\&\, \neg B$ 1′ and 2′.
4. $\neg A \,\&\, \neg B$ 3 and de Morgan	
5. $\neg B$ 2 and 4	
6. $A \,\&\, \neg B$ 2 and 5.	

We have proved that the *LHS* and the *RHS* have the same implicates. To prove that these in turn imply the original propositions we proceed as follows.

LHS	*RHS*
7. $B \vee \neg B$ 5 and addition	4′. $(A \,\&\, B) \vee (A \,\&\, \neg B)$ 3′ and addition.
8. $A \,\&\, (B \vee \neg B)$ 2 and 7.	

In short, the distributive law is seen to hold when the tacit assumption concerning the mutually "complementary" propositions is rendered explicit. Hence the second motivation for playing with logics characterized by non-distributivity, in particular quantum logics, evaporates as well.

The upshot of the preceding discussion is that quantum logics are not well motivated. (Moreover they are mathematically suspect as well, for every lattice endowed with implication, i.e. every Brouwer algebra, is distributive: see Rutherford 1965 p. 74.) We could have arrived at our result in a far quicker way, e.g. by pointing out that (*a*) so far nobody has ever employed a quantum logic to derive a correct result in quantum theory, and (*b*) work in quantum logic over half a century has produced no new scientific results. The explanation for this lies in the fact that quantum theories contain only mathematical theories the underlying logic of which is classical, as can be seen only by axiomatizing quantum mechanics (Bunge 1967c). So much, for the time being, for the motivations of non-standard logics.

Let us now tackle the third problem in our agenda, *viz.*, *What can be done with deviant logics that cannot be done with standard logic*: i.e., which kinds of new problems can they help solve? At first sight the answer is simple enough: A logical system that is restricted (extended) with respect to standard logic allows one to pose a subset (superset) of the problems that standard logic can handle. This answer is correct as far as the *logical* problematics goes. For example, a negation-less logic cannot handle any problems involving negation.

However, that simple answer is wrong with respect to the *mathematical* problematics. Indeed, a restricted logical system, provided it is not too poor, allows for more freedom to mathematical inventiveness because it imposes less restrictions. For example, intuitionistic logic, by not including the theorems of double negation, excluded middle, and de Morgan, underlies certain mathematical theories disallowed by ordinary logic. (An important recent example is topos theory.) However, such enrichments in some departments of mathematics are paid for by severe curtailments in others. Therefore it would be rash to either impose or outcast intuitionistic logic: a moderate pluralistic foundational policy seems advisable. See Sect. 5.1.

The situation with regard to the systems of extended logic is the dual of the preceding: they allow one to pose more logical problems but, precisely because they are richer (more specific), they restrict mathematical inventiveness. Thus modal logic is more comprehensive than classical logic, but mathematicians have found no use for it except as an object of metamathematical study. And even the worth of the additional logical riches gained by the extended logical systems is doubtful. Recall that modal logic and other extended logical systems failed to solve the problems they set out to solve. Those problems are best tackled elsewhere – e.g. in model theory, semantics, epistemology, or ontology.

In short, the only deviant logic that has proved to be of use to mathematics is intuitionistic logic – as long as it is separated from its philosophical concomitants. The remaining non-standard logics have proved useless and have no sound philosophical motivations. This rather negative evaluation of deviant logics does not entail that they have been barren. On the contrary, they have sparked off some interesting work in algebra (e.g. Rasiowa 1974). And, more important to us, they have helped us – albeit at a staggering price – to better understand the nature of logic and even of rationality. In particular deviant logics have taught us that (*a*) whatever else it may be, formal logic is not the science of the laws of thought; (*b*) the logical principles, far from being eternal truths, are our own brain children and, as

such, subject to some change in the course of history – though we must keep some invariants if we are to continue to study the same subject and retain rationality; (*c*) it is possible to reason in numerous non-classical ways – e.g. intuitionistically or modally – without incurring contradiction; (*d*) however queer a logical theory may be – even if it casts off the principle of contradiction – it can be formalized, whence formalization does not guarantee exactness, consistency, or usefulness: it only helps attain them; (*e*) formalization is necessary but insufficient to handle topic-dependent problems, such as those of reference (which motivated relevance logic) and change (which motivated temporal logic).

In view that all of the logics proper are equally rigorous, i.e. precise and consistent, it might be thought that our preference for the two main systems – classical and intuitionistic logic, and particularly the former – is sheer conservatism. Granted: if rigor (formal correctness) were the only choice criterion, then we ought to be logical relativists or pluralists, as Rescher (1977 a Chs. xiii and xiv) has argued with his usual force. But in fact mathematicians, the masters of contemporary logic, are more exacting: they demand that a system of logic *stricto sensu* satisfy all of the following admissibility conditions:

(i) A system of logic should be precise: it should contain only exact concepts;

(ii) a system of logic should include all the basic operations of formal reasoning (such as negation and conjunction, "existential" quantification and universal quantification), it should contain the modus ponens and – last but not least - the principle of contradiction;

(iii) a system of logic should be able to analyze and systematize the inference patterns that have been found successful in mathematics and in factual science; and preference should be accorded the system which best captures the subtleties of mathematical and scientific reasoning, and which keeps abreast of mathematical and scientific advances;

(iv) no system of logic is admissible which condones fallacies (such as affirming the consequent) or generates insoluble paradoxes (to be distinguished from merely counterintuitive propositions);

(v) no system of logic is admissible which impoverishes mathematics or science to the point of precluding the formulation of well worn theorems or law statements, or which misguides people by suggesting that logic can solve by itself problems in semantics, epistemology, ontology, ethics, or factual science.

I submit that classical and intuitionistic logic are the only known logical

theories satisfying all of the above conditions. (Moreover classical logic is the only one that harmonizes with the bulk of mathematics and factual science. This is no mean feat for, if a different logic were to be adopted, the whole of factual science would have to be renovated in order to jibe with this modified mathematics.) Finally, classical and intuitionistic logic are far from being stagnant bodies of knowledge, so the charge of conservatism is groundless. In fact they have been moving so fast these past decades that anyone who got his training in logic twenty years ago, and has not kept up to date, would be unable to read the current issues of the logic journals. In conclusion, unrestricted logical relativism (or pluralism) does not pay: logical monism does, and dualism may.

We are now ready for our final question: *What becomes of rationality if there is a plurality of formally correct systems of logic*? The answer to this question depends critically upon the definition of "rationality" that one may care to adopt. Now, the very first problem we meet when attempting to define the notion of rationality is that it is not unique. Indeed one must distinguish at least the following seven concepts of rationality (Bunge 1985):

(i) *conceptual*: minimizing fuzziness (vagueness, imprecision);

(ii) *logical*: striving for consistency and giving reasons for every claim;

(iii) *methodological*: questioning and demanding proof or evidence;

(iv) *epistemological*: avoiding conjectures incompatible with the bulk of background knowledge;

(v) *ontological*: adopting a self-consistent naturalistic world-view;

(vi) *valuational* (Weber's *Wertrationalität*): striving for goals that, in addition to being attainable, are worth being attained;

(vii) *practical* (Weber's *Zweckrationalität*): adopting means likely to help attain the goals in view.

I submit that these various kinds of rationality are ordered in the way they appear above. (See Mosterín 1978 for the thesis that practical rationality presupposes theoretical rationality.) Accordingly, conceptual and logical rationality are necessary for rationality of any other kind: they compose *minimal rationality*. If we stick by the latter we shall avoid or refine fuzzy predicates, which do not satisfy classical logic; and we shall avoid or remove inconsistency, which hinders the search for truth.

Our definitions of conceptual and logical rationality condemn paralogics – in particular paraconsistent, nonsense, and fuzzy logics – as irrational. Indeed, by condoning fuzziness or inconsistency they break the paramount rule of the logical game. (If you wish your game to be called 'soccer', abide by the rules of soccer.) In particular, paraconsistent logic tolerates empty

concepts, such as the set of all the objects that possess and fail to possess a given property. And it tolerates, nay encourages, inconsistent theories, which contain mathematically nonexistent objects (recall Sect. 2.1). (Moreover an inconsistent theory is really a set of at least two mutually incompatible systems, every one of which can be supported by some body of "soft" data.)

In our view paralogics promise only intellectual anarchy: the destruction of all kinds of rationality. The paralogician, dazzled by formalization – which is normally but a means – has lost sight of the job of logic: he has abdicated the logician's duty, which is to prevent reason from going astray, and to help us reduce fuzziness and avoid inconsistency.

On the other hand our definitions of conceptual and logical rationality do not exclude any deviant (but consistent) logics, whether restricted, extended, or neither. All of the deviant logics spare rationality although they draw differently the borders of reason. The problem is to decide whether we need or want a shift in the frontiers at this time. And this is not a logical problem but one involving mathematics, factual science, and philosophy. We now have the means to solve that problem. Our answer is this:

(i) *we do not need* the extended and connexive logics because in some cases they do not solve any significant problems other than those solved by standard logic, in others they do not solve correctly the problems they were intended to solve, and in still other cases they attempt unsuccessfully to grapple with problems belonging to other departments, notably semantics;

(ii) *we do not want* the restricted logics – except possibly the least restrictive of all, namely intuitionistic logic – because they mutilate important branches of the tree of knowledge;

(iii) *we need and want* to stick to classical logic, or at least to intuitionistic logic, because these systems

(*a*) satisfy all of the admissibility requirements listed a short while ago - essentially the ability to exactify, analyze and systematize all the successful inference patterns;

(*b*) have proved useful not only as tools of analysis and canons of discussion, but also as tools for reconstructing intuitive mathematics in a rigorous way, and occasionally also for building new mathematical theories (as was recently the case with non-standard analysis, an offshoot of model theory, and with topos theory, an offshoot of category theory);

(*c*) satisfy the requirements of conceptual, logical, epistemological and methodological rationality.

At the time of writing, standard (classical) logic has the additional

advantage that it underlies the bulk of mathematics and therefore of all the factual sciences. (Intuitionistic logic underlies only a comparatively small portion of mathematics, and it has yet to be adopted by any factual theories.) And logical unity is of paramount importance because it makes it possible for the various research fields to interact with one another – an interaction that promotes the unity (or systemicity) of human knowledge. The moment one branch of learning were to adopt a non-standard logic it would cease to import and export ideas: it would become insular and so, eventually it would degenerate into a pseudoscience. (Recall condition (xi), in Sect. 1.1, for a branch of knowledge to be scientific.) For example, if quantum mechanics were to adopt a logic of its own it would cease to interact with classical physics and would thus become untestable. (This should suffice to write off quantum logics.) For the time being intuitionistic logic does not comply with the unity of knowledge requirement: it is marginal. This situation is tolerable only as a transient state. Eventually the whole of mathematics will have to be built on the basis of either classical or intuitionistic logic – or of a third still unknown system.

To conclude. Non-standard logic is a variegated field of exact if largely idle research. Further developments in it are unavoidable given the curiosity and lust for generality of mathematicians, as well as the fact that many philosophers with an exact cast of mind have a tendency to tackle epistemological, ontological and ethical problems in an a priori manner, producing "logics" of change, belief, preference, and much else. However, after nearly a century, non-standard logic can exhibit but one useful system, namely intuitionistic logic. The remaining non-standard calculi are just intellectual games that have failed to solve any philosophical, mathematical or scientific problems.

4. Pure and Applied Mathematics

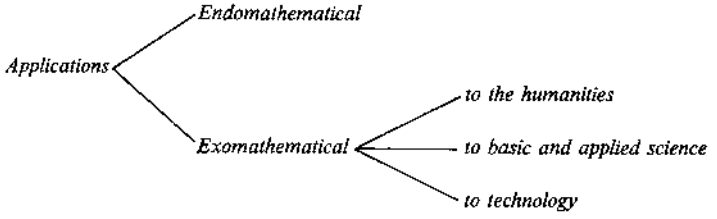
4.1. *Applications of mathematics*

Every mathematical idea can be worked out either for its own sake or in view of some application. Moreover, in principle every mathematical idea can be used in contexts other than that in which it originated. Indeed, mathematics and philosophy are the most diffusive or portable of all fields of knowledge.

However, not all applications of mathematics are to extramathematical fields. Thus number theory, which originated in counting, developed into a branch of pure mathematics which was eventually applied to geometry and

other branches of pure mathematics. Group theory, which originated in algebra, was later applied to geometry and factual science, mainly physics. On the other hand geometry, which originated in land surveying, became a branch of pure mathematics which in turn found uncounted applications in factual science and technology. Likewise the infinitesimal calculus, which was partly conceived as the mathematics of motion, evolved into a theory in its own right and was eventually applied not only in natural science and technology but also in economics. There is a constant give and take between pure and applied mathematics.

The above examples suggest classing the applications of mathematics into *endomathematical* and *exomathematical*. Thus the use of set theory to reformulate mathematical analysis in a more rigorous (and less intuitive) fashion is an example of an endomathematical application. On the other hand the following are instances of applications of mathematics to other disciplines: (*a*) to the humanities: using the notion of a function to elucidate that of reference, and that of a semigroup to exactify that of exact language; (*b*) to factual science: using manifold geometry in the theory of gravitation, and probability theory in the theory of social mobility; (*c*) to technology: using queueing theory to design telephone exchanges, and probability theory to design quality control procedures. In sum, we have the following systematics of the applications of mathematics.



Because some applications of mathematics are done within mathematics – as is the case with the algebraic treatment of logic – the collection of applications of mathematics is not identical with what is usually called 'applied mathematics'. (I.e. some applications of mathematics remain within pure mathematics.) Unfortunately the expression 'applied mathematics' is ambiguous. In some contexts it stands for the study of mathematical problems that happen to arise in research fields other than mathematics, though divested of their factual content. This is the case with queueing theory, the general theory of control, and potential theory. (The latter, a part of mathematical physics, was born from the classical theory of gravitation,

which is in turn a branch of theoretical physics.) In other cases 'applied mathematics' designates the collection of exomathematical applications of mathematics. In this sense exact philosophy and a mathematical model of an ecosystem may be said to belong to applied mathematics.

The difference between the two senses of 'applied mathematics' is one of reference and of attitude or interest. In the first case the applied mathematician is interested in mathematics *per se*, and consequently he produces mathematical theories or theorems that have no precise reference. (For example automata theory, though initially motivated by computer science, can also be worked on as a branch of pure mathematics.) In the second case the applied mathematician is not primarily interested in mathematics but uses the latter as a tool for thinking about some nonmathematical object – such as the concept of truth, or a thermonuclear reaction, or a change in the frequency of a genetic trait – or for controlling some natural or social process. He is a humanist, scientist, or technologist, who handles mathematics as a tool. Let us concentrate on this second sense of 'applied mathematics'.

Consider first the most basic branch of mathematics, namely logic *lato sensu*. Applied logic is of course the application of the concepts and principles of logic to nonlogical problems. There have been two extreme stands with regard to the scope of such application: nihilism and panlogicism. The nihilists, echoing Pascal's distinction between the *esprit de géométrie* and the *esprit de finesse*, believe that logic is just shorthand (symbolization), or trivial routine like doing sums (Ryle), or a caricature of ordinary language. Ordinary language philosophers and irrationalists have made these claims.

All three claims are grossly in error. The first, because logic is a set of theories not just a symbolism. The second, because new, ever more difficult problems, keep cropping up in logic. The third, because logical analysis has been able to exactify uncounted coarse ordinary knowledge notions and reasonings. Only logic (and more generally mathematics) is imbued of the *esprit de finesse*. Ordinary knowledge and its medium, ordinary language, embody the *esprit de vulgarité*.

The optimists concerning the scope of logic believe, on the other hand, that the logician *qua* such is competent to approach any problems, conceptual or empirical, philosophical or scientific. There are still logicians daring enough to tilt at any problems with the sole help of the logical lance: witness the unchecked proliferation of deviant extended logics dealing with such factual items as becoming. (See Sect. 3.2.) Still, some philosophers realize

that such attempts are doomed to failure for lack of contact with factual science.

I submit that the main exomathematical applications of logic are to philosophical problems, and even so to problems of a restricted kind, namely of philosophical analysis – in particular the analysis (or exactification) of philosophical concepts and propositions, and the analysis of reasoning, dialogue, and disputation. On the other hand philosophical synthesis, or the construction (or reconstruction) of philosophical theories in ontology, epistemology, ethics, or other fields, should be out of bounds because it requires not only more powerful formal tools (such as abstract algebra and topology), but also some substantive knowledge (e.g. in physics or in biology). Thus a theory of mind that were to use only the predicate calculus and our commonsense knowledge about mental phenomena would be hopelessly shallow and out of touch with the science of mind. Far from being a contribution to knowledge, it would merely be an intellectual pastime.

We adopt then a moderate stand with regard to the scope of applied logic. But even within this moderate stand one may feel that there are limits, or that there are none, to the exactification of concepts. Pessimists feel that there are certain inherently confused or vague notions bound to resists all efforts at mathematization. One such notion would be that of impetus, the cornerstone of a non-Aristotelian dynamical theory that flourished between the 6th and the 16th centuries (Koyré 1943). But this thesis is debatable. After all, the impetus – which was supposed to keep bodies moving after they had been set in motion – resembles our modern concept of inertia (mass). So, it is conceivable that a competent mathematician could have exactified it – though not in isolation but as a member of a hypothetico-deductive system.

Any interesting notion is worth an exactification effort. (But of course it won't pay to use heavy artillery to kill flies – e.g. to use differential equations to account for yearly economic statistical figures.) To drop an interesting concept for being vague is defeatism, because all new concepts are born fuzzy. And to wait for it to be clarified without the help of mathematics is no better than believing in miracles. The right policy with regard to the bounds of exactification is summarized in

RULE 1.1. Try to exactify every interesting concept, if necessary embedding it in a mathematical theory. And push the exactification effort as long as it is worthwhile.

Not everyone believes in exactification. In fact its opposite, or fuzzification, is now quite trendy among applied mathematicians. It consists in systematically replacing ordinary sets with fuzzy sets – or, equivalently, in substituting the relation of graded membership for that of ordinary set membership. This fuzzification program (Zadeh 1965) has produced a large number of more or less predictable generalizations, to the point of having been dubbed a growth industry (Arbib 1977).

(The concept of a fuzzy set is usually regarded as a generalization of that of ordinary set. However, it can be shown to be reducible to that of a family of ordinary sets. Recall how the concept of graded membership is introduced. One starts off with the characteristic function χ_A of a subset A of a given universe U:
χ_A: $U \to \{0,1\}$, such that $\chi_A(x) = 1$ iff $x \in A$, and $\chi_A(x) = 0$ iff $x \notin A$.
One then generalizes the range of the characteristic function to the entire real interval $[0,1]$, or some other structure, and introduces the notation

$$x \in_v A \text{ iff } \chi_A(x) = v, \text{ where } v \in [0,1].$$

The *LHS* is read "the degree of membership of x in A is v". This degree is left undefined: it is simply assigned on some extramathematical ground. However, this fuzziness can be removed as follows. Take the power set 2^A of A – where A is again a subset of a given universe U – and ordinary membership in every member of that family of sets. Moreover, assign every subset V of A a certain measure $\mu(V)$ normalized to unity. The concept of graded membership can then be defined in terms of the concepts of ordinary membership and measure, namely thus:

$$x \in_v A =_{df} [\chi_A(x) = v, \text{ with } v = \mu(V), \text{ iff } x \in V \in 2^A].$$

Clearly, this definition holds only for μ-measurable sets. In this case there are as many membership degrees as there are sets, with different measures, in 2^A. In particular, $v = 1$ for $V = A$, and $v = 0$ for $V = \varnothing$. This completes our exactification of membership fuzziness.)

Let us now move on to the typical activity of the mathematician engaged in exomathematical applications, namely *model building*. To begin with let us consider mathematical models in philosophy. There are none in logic *stricto sensu* because the logical theories are maximally general, whereas models are specific. Philosophical models are similar to models everywhere else in being specific theories. In fact a philosophical model is a mathematical theory about a circumscribed class of philosophical concepts. Examples: our own theories of reference, sense, and meaning (Vols. 1 and

2), and of systems, spacetime, and mind (Vols. 3 and 4). On the other hand most of our views in Vols. 5 and 6 are at best condensation nuclei for mathematical models. (And, of course, what philosophers call the 'covering law model' is a thesis, and 'Nagel's model of reduction' is a definition.)

A possible formalization of the notion of a philosophical model is embodied in

DEFINITION 1. A *philosophical model* is a theory about a triple

$$\mathcal{M} = \langle D, F, I \rangle,$$

where

(i) D, the *domain*, or reference class of $\mathcal{M}$, is a a set of concepts or entities of philosophical interest;

(ii) F, the *formalism* of $\mathcal{M}$, is the union of certain theories in pure mathematics (e.g. predicate logic, group theory, and elementary point set topology);

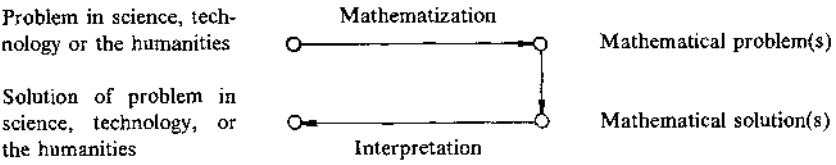
(iii) I, the *interpretation* of $\mathcal{M}$, is a partial function from the formalism F to the power set of the domain D, that assigns to some predicates and formulas in F sets of concepts or entities in D.

Example 1 The probability theory of truth interprets the probability of a proposition as its degree of truth. (It does not work: see Vol. 2, Ch. 8, Sect. 4.1.) *Example 2* The probability theory of causality interprets the conditional probability of an event given that another event happens, as the strength of the causal link between the two. (It does not work: see Bunge 1982a) *Example 3* Automata theory, enriched with a set of suitable semantic assumptions, is a simple (hence highly idealized) model of things that are at the mercy of their environment (Bunge 1973b, Ch. 8, Sect. 5).

Like art, mathematics can be *figurative* (or representational) or *non-figurative* (or "abstract"). Thus, whereas the models in our semantics are nonfigurative, those in our ontology are figurative (though symbolic rather than iconic) in the sense that they refer to concrete entities. All of the mathematical models in factual science and technology are likewise figurative in this sense – though they are rarely pictorial. This is one of the advantages of modern man over his predecessors: that he can approach reality equipped with a huge tool box full of general (hence portable) ideas, mainly those of mathematics. Hence the difference in outcome, namely mathematical models in addition to pictures and verbal descriptions. (Imagine, if you can, what the dwellers of the caves of Altamira or Lascaux would have painted, had they bred bisons instead of hunting them, and built mathematical models of bison farms.) In short, we can approach the study

and control of reality with a rich fund of formal ideas, i.e. of ideas that do not describe reality but provide the necessary framework for models that do describe concrete things.

Every applied mathematician is familiar with this flow diagram or some modified version of it:



The diagram is obvious enough but it poses the philosophical problems of elucidating the notions of mathematization and interpretation. The latter is a semantic concept that was tackled in Vol. 2, Ch. 6, Sect. 3. As for the concept of mathematization, it may be construed as the ordered couple composed of deinterpretation (gutting of factual content) and exactification with the help of some mathematical tools. The actual mathematization and interpretation processes may be quite involved. To begin with, mathematization is never unique, although it may look to be when we have only a few tools in our mathematical kit. As for the interpretation difficulties, suffice it to recall the controversies over the mass concept in the 18th century, the energy concept in the next, and the state function in quantum theory in our own days. But the problems posed by mathematization and interpretation can be revealed at a much more elementary level, as shown in the following examples.

Example 1 It is often stated either that it makes no sense to add three peaches to four quince, or that it does make sense, the result being exactly three peaches plus four quince. This pseudoproblem is dissolved by recalling that peaches and quince happen to be fruits, so that the problem makes perfect sense and has a nontrivial solution, namely seven pieces of fruit. (To indulge in pedantry, call $p \subseteq P$ a set of peaches, and $q \subseteq Q$ a set of quince. Further, call $f \subseteq F$, where $F \supseteq P \cup Q$ is a set of fruits including all peaches and quince. Then set $f = p \cup q$, whence $|f| = 7$ since by hypothesis $|p| = 3$, $|q| = 4$.) *Example 2* At first blush combinatorics is a sort of general physics (or ontology) for dealing with all the possible arrangements or configurations of sets of objects of an arbitrary nature. For example it tells us that, no matter what the nature of three objects a, b and c may be, there are just $3! = 6$ ways of arranging them linearly to produce the chains

abc, *acb*, *bac*, *bca*, *cab*, and *cba*. However, these are just logical or conceptual possibilities, not physical ones. In fact it may well be that the given objects cannot assemble in any way, or only in one way. That is, the number of nomologically possible arrangements need not be 6 but, depending on the nature of the objects, some integer lying between 0 and 6: this number will be determined by the interactions among the components as well as among these and their environment. *Example 3* Consider the usual mathematization of Zeno's paradox of the race course: it is the infinite series with general term $(1/2)^n$. The series adds up to 1, so there is no paradox if this series is regarded as just a mathematical analysis (one out of infinitely many) of the unit distance. The paradox arises only because Zeno is an operationist: he insists on interpreting every one of the infinitely many terms of the series as an action (running). There are similar paradoxes in quantum mechanics and they are dissolved by avoiding operationism: see Ch. 2, Sect. 6.1.

A while ago we clarified the notion of a philosophical model. Let us now elucidate that of a factual model, or model of things or processes (natural or artificial) of some kind:

DEFINITION 1.2 A *factual model* is a theory about a triple

$$\mathcal{M} = \langle D, F, I \rangle,$$

where

(i) D, the *domain* or reference class of $\mathcal{M}$, is a set of factual items, i.e. concrete things or processes in them;

(ii) F, the *formalism* of $\mathcal{M}$, is the union of some theories in pure mathematics;

(iii) I, the *interpretation* of $\mathcal{M}$, is a partial function from the formalism F to the power set of the domain D, that assigns some predicates and formulas in F sets of factual items in D.

It is convenient to distinguish two kinds of factual model: scientific and technological, according to whether it represents naturally existing things or processes, or man-made ones. (A model of a chemical reaction is of the first kind, whereas a production model of a firm is of the second.) So, we make

DEFINITION 1.3 A factual model about a triple $\mathcal{M} = \langle D, F, I \rangle$

(i) is a *scientific model* iff all the elements of D are natural or social objects, and it complies with the general requirements of science (Vol. 6, Ch. 14, Sect. 2.1);

(ii) is a *technological model* iff some of the elements of its domain D are

concrete artifacts or plans for human action, and it complies with the general requirements of technology (Vol. 6, Ch. 14, Sect. 2.2).

Finally we are ready for

DEFINITION 1.4 At any given time the fund of knowledge of *applied mathematics* is the collection of scientific or technological models extant at that time.

There is often talk of 'the application of mathematics to reality' and, since Leibniz, some scholars have marveled at the apparently perfect fit of mathematics to reality. Actually there is no such application of mathematics to *reality*: all there is, is a mathematization of some of our *ideas* about reality. More precisely, when building a mathematical model of some concrete thing, be it electron or industrial plant, one builds or borrows a mathematical formalism, and interprets some of its concepts in factual terms. (Recall Vol. 2, Ch. 6.) By itself, the mathematical formalism is ontologically neutral, this being why we can use it again and again in different research fields.

The ontological commitment of a mathematical model of a piece of reality lies not in its mathematical formalism but in the semantic assumptions that specify the kind of thing that the model refers to and what properties of such things some of the concepts in the model represent. So much so that one and the same mathematical formalism may be attached alternative sets of semantic assumptions. (For example, a probability may be interpreted as the strength of the possibility of an atomic transition in one model, as that of a genic mutation in another, and as that of an error in the transmission of a message in a third.) The main interest of semantic assumptions to the philosophy of mathematics is that their presence or absence is what distinguishes applied from pure mathematics. (More on semantic assumptions in Vols. 1 and 2.)

A subsidiary interest of the consideration of semantic assumptions as the trademark of applied mathematics is that they solve, or rather dissolve, the problem of "the right" mathematics for dealing with problems of a certain kind, e.g. the description of change. Until recently it was widely believed that there is a special mathematics of change, namely the infinitesimal calculus. In recent times it has been claimed that category theory, with its focus on arrows (morphisms) rather than on sets, is the basic tool for the description of change. Both opinions are wrong, as shown by the following example. According to Archimedes' principle in hydrostatics, a balloon will move downwards only if its weight exceeds the buoyancy exerted by the air. Here we are describing (and even explaining) change without the help of either the

calculus or category theory. There is no such thing as the mathematics of change as opposed to that of rest: there are only factual theories, some dynamical, others static, that include mathematical formalisms. (Moreover sometimes we know enough mathematics to build alternative but empirically equivalent models of a given process.) Change, or its absence, is described by a mathematical formalism enriched with semantic assumptions. In the case of Archimedes' principle these are: (*a*) The body of weight W is in equilibrium with the surrounding fluid iff $W = B$, where B is the buoyancy exerted by the fluid on the body; (*b*) the body moves upwards iff $W < B$, and downwards iff $W > B$.

As for the alleged perfect fit of mathematics to reality, Platonists have no difficulty in accounting for it: they state that every "form" (or at least every "significant mathematical formula") *must* have a counterpart in the real world, even if we have yet to find it. (See e.g. Thom 1975.) But this is just a pious hope. Moreover it explains a nonfact, for every mathematical model is defective in some way or other: it is at the very best a close approximation rather than perfectly true, for it involves some idealization, such as the neglect of some variables representing properties of the thing concerned or of those in its environment. In sum there is no perfect fit of mathematics to reality.

A related but this time genuine problem is Wigner's (1960): How is it possible for concepts without a definite factual reference (the psychologist's "intervening variables") to be necessary in theories that describe satisfactorily scientific observations? This problem is not solved by assuming that there is no difference between pure and applied mathematics, or that there is a pre-established harmony between the mind and the external world. Rather, it is solved by recalling the following points. Firstly, when a mathematical theory is summoned to help solve a scientific or technological problem, only a small portion of it is used explicitly: the rest remains unused as the support of the part that is being explicitly utilized. For example, in solving an ordinary differential equation one assumes tacitly the theorems that guarantee the existence and uniqueness of its solution. (Analog: When driving a car we pay attention to only a few inputs and outputs.) Secondly, every representation of a chunk of reality – be it a mathematical model or a painting – is symbolic and, as such, it has some conventional components. (For example, the choice of coordinate system and of units is conventional. For conventional elements in the plastic arts see Gombrich 1961.) Thirdly, although there are individual differences among model builders, they all share a basic common neuronal organization and are steeped in roughly the same cultural tradition.

Another question asked, particularly by practically-minded people, is this: What makes a mathematical construct applicable? For instance, why have only a few mathematical geometries found application in physics? It won't do to reply that certain constructs just happen to be inapplicable. If there are indeed any such constructs then we still wish to know why they are inapplicable – in view of the Platonic, empiricist, and pragmatist claim that there can be no such constructs. The truth is that "We don't know what will be useful (or even essential) until we have used it. We can't rely upon the concepts and techniques which have been applied in the past, unless we want to rule out the possibility of significant innovation" (Browder and MacLane 1978 p. 348). All we know is that a person may use mathematical tool *X*, sooner or later, only if *X* happens to be in his tool kit or he has heard of *X*.

Mathematics is necessary but insufficient to build mathematical models of some cognitive or practical value in science or technology. In addition, some substantive knowledge and some flair (intuition or "nose") are needed. Otherwise the models will be far off the mark: they will be just mathematical toys. Unfortunately there is a certain tendency among applied mathematicians to play mathematical games, i.e. to investigate neat academic problems instead of grappling with the complexities and dirt of the real world. That tendency is particularly obnoxious in the social sciences, where many a mathematical model is based on more or less plausible (commonsensical) but entirely arbitrary assumptions that entail "precisely stated but irrelevant theoretical conclusions" (Leontief 1982). The net result is that many of the scientists interested in understanding the object of their study are turned away from mathematics.

It has been rightly said that mathematics is a good servant but a bad master. That is, in factual science and technology mathematics should be handled as an instrument to build realistic models solving genuine problems. (This is the goal. Initially it may be necessary to start with brutal simplifications, such as one-dimensional or static models of things.) If the mathematician wants to enjoy utter freedom, in particular freedom from empirical constraints, he must turn to pure research. Here he can invent or generalize almost any construct. In applied mathematics, on the other hand, "the emphasis is on formulae that can be used to answer specific problems rather than on proofs of theorems under conditions of the utmost generality" (Cox 1962 p. ix).

The difference between pure and applied mathematics does not prevent their practitioners from interacting vigorously. It is a matter of historical

record that S & T have been a source of mathematical problems, and pure mathematics a store of ready-made conceptual tools that scientists and technologists have always felt free to use. Sometimes mathematical models of real things are built by using well known mathematical tools, at other times the latter have to be wrought *ad hoc*. And occasionally one may start by building a mathematical formalism, only subsequently looking around for applications. Thus one of the founding fathers of quantum theory stated that a good deal of his research work consisted in "simply examining mathematical quantities of a kind that physicists use and trying to fit them together in an interesting way regardless of any application that the work may have. It is simply a search for pretty mathematics. It may turn out later that the work does have an application. Then one has had good luck" (Dirac 1982 p. 603). The mission-oriented mathematician is not allowed such leisurely exploration of ideas; consequently he cannot expect any such "lucky" strikes.

To conclude. Pure mathematics has no ontological commitment: it does not refer to reality. (If it did then it would constitute the a priori science of the world.) However, it is the basic language of S & T as well as its conceptual sharpener and deductive machinery. In other words, mathematics constitutes no factual knowledge but it is an indispensable means for acquiring precise and profound factual knowledge. This conception of the nature and role of mathematics may be called *instrumentalist formalism*, or *formal instrumentalism*. It differs from the instrumentalist (or pragmatist) epistemology in that (*a*) it does not state that mathematical formulas are rules or instructions rather than propositions, (*b*) it does not make practice the value criterion, and consequently (*c*) it does not reject the mathematical ideas that have not yet found application, any more than those that are no longer widely used in S & T.

4.2. *An Example: Probability*

No concepts have been greater sources of inspiration and trouble, for mathematicians and philosophers alike, than those of infinity, space, and probability. Fortunately most of the fog surrounding these concepts has been dissipated in the course of the last hundred years or so. Cantor taught us that (actual) infinity is an entire (infinite) family of concepts forming the so-called hierarchy of transfinite numbers. Geometers and topologists have created uncounted spaces, and Einstein taught us to distinguish these constructs from (the unique) physical space, and correspondingly the mathematical geometries from physical geometry. (Recall Vol. 3, Ch. 6 and see

Torretti 1978.) The problem of probability differs from that of space in that there is essentially a single core mathematical concept of probability (though the general theory defines an infinite class of probability measures). The analogy between space and probability lies in that both can be variously interpreted. In fact there are at least five different probability concepts counting the one elucidated by the pure or basic theory of probability. Let us sketch them following Bunge (1981b).

Probability theory is a branch of pure mathematics and, more particularly, a chapter of measure theory. In fact a probability measure is a real valued and bounded function P defined on a family A of *abstract* sets. The only conditions that A must satisfy – i.e. the ones that guarantee its (formal) existence – are the following purely formal and rather mild ones: (*a*) the complement of every member of A is in A; (*b*) the intersection of any two members of A are in A, and (*c*) the countable unions of any elements of A are in A. (In sum, A must be a sigma algebra.) The function P is specified by two or more axioms, depending on the theory. (See Fine 1973 for a review.) For example, in Kolmogoroff's theory two axioms suffice to define (implicitly) the concept of absolute probability, whereas in Renyi's three axioms are required to define the concept of conditional probability.

Nothing is said in these theories about events (except in the Pickwickian sense of being members of the family A) or their frequencies, let alone about methods for calculating or measuring probabilities. These other notions occur in the applications. The various probability theories are semiabstract, in the sense that (*a*) they do not specify the nature of the elements of the probability space A, whereas (*b*) they do specify the range of the probability measure, which is the unit interval of the real line rather than another abstract set.

Were it not for this partial semantic indeterminacy, the probability calculus could not be applied almost everywhere, from physical science and physiotechnology to social science and sociotechnology. As long as the probability space A is left uninterpreted, probability has nothing to do with anything extramathematical: $P(x)$, where x belongs to A, is just a number. This number is interpreted as the strength of the possibility of x only upon interpreting A as a collection of facts. Therefore any attempts to define the general concept of probability in specific terms, such as those of favorable case, or degree of belief, or relative frequency in a sequence of trials, is bound to fail. These concepts did have some heuristic value in the past, and they occur in some applications, but they have no place in the basic theories of probability, which are maximally general. Having secured the general and

rigorous concept we may hope to apply it to a wide range of problems.

An *application* of probability theory is a factual model of the kind discussed in Sect. 4.1: it is a probabilistic model of the factual items concerned. More precisely, we make

DEFINITION 1.5 A factual model about a triple $\mathscr{M} = \langle D, F, I \rangle$ is a *probabilistic model* of the domain D of factual items iff

(i) the formalism of $\mathscr{M}$ contains a probability calculus or part of it, and

(ii) the interpretation I of $\mathscr{M}$ specifies (*a*) that the domains of the probability measures occurring in F are included in the domain D of factual items (states of things or events in things), and (*b*) the probability $P(x)$ of every factual item x measures the chance, or degree of possibility, or propensity of x.

DEFINITION 1.6 A collection D of factual items is called *probabilistic* (or *stochastic* or *random*) iff there is a (sufficiently) true probabilistic model of D (so that every element of D is assigned a definite probability) that is nontrivial (i.e. such that there is at least one probability value different from either 0 or 1).

Contemporary science and technology are chock full of probabilistic models, many of which are true to fact at least to a first approximation. In other words, science and technology acknowledge the existence of a large number of probabilistic (or stochastic or random) collections of factual items, i.e. of things that behave probabilistically. Suffice it to mention but a few well known probabilistic models: of coin flipping and of lotteries; of elementary "particles", atomic nuclei, atoms, molecules, and crystals; of genic mutation and of evolution; of the transmission of signals along nerve fibres or information networks; of learning and of social mobility.

Our Definitions 1.5 and 1.6 contain a covert assumption: they assume that *the* correct interpretation of a probability space A is that of a collection of (random) *factual items*, and that of $P(x)$, for every x in A, is that it quantitates the *objective possibility* of x. (To put it negatively: neither A nor P have anything to do with subjectivity – except of course that both are assumed or checked by some knowing subject or other.) In other words, the two definitions summarize the *objectivist* or *realistic* interpretation of probability. This view is of course in line with any realistic epistemology (Vols. 5 and 6), but at variance with alternative philosophies of knowledge. Let us justify that interpretation and criticize its rivals.

The realistic or objectivist interpretation of probability can be found in

Poincaré (1903), Smoluchowski (1918), Fréchet (1946) and a few other scientists. (In particular, Fréchet regarded probabilities as physical properties on the same footing as distances or masses.) This interpretation was reintroduced independently by Bunge (1951) and by Popper (1957), and adopted, in some guise or other, by several other philosophers (see Settle, 1974). My own favorite arguments in support of it are as follows.

Firstly, when wishing to make sure that choice or preference will play no role in making some decision, we flip a coin or use a table of random numbers. This procedure presupposes the hypothesis that chance is objective and, more precisely, a property of every one of the individual items forming the basic probability space A.

Secondly, in many cases probabilities can be measured, just like any other substantial properties, with the help of objective indicators. For example, counting the long run frequency of repetitive events of a given kind one can estimate their probability. And measuring the intensity of a monochromatic light source allows one to estimate the probability of the transitions of its atoms from one state to another.

Thirdly, in many a probabilistic model occurring in science and technology the probability space A can be built out of the state space of things of the kind being studied. For example, one may set A equal to the power set of the state space. In this way every member of the probability space A is a bunch of states, and $P(x)$ becomes the strength of the tendency (or propensity) the thing has to dwell in the state or states x. Similarly, if x and y are states (or sets of states) of a thing, the conditional probability of y given x, i.e. $P(y|x)$ is interpreted as the strength of the tendency or propensity for the thing to go from state(s) x over to state(s) y.

Fourthly, the whole point of any probabilistic model in science or technology is to account for the behavior of objectively real things. So much so that all the probabilities occurring in them are probabilities of states or of changes of state observed or assumed to occur in external things. Thus when a physicist calculates or measures the probability of an atomic collision of a certain kind, he refers to an event in the real world, not to his own state of mind; and he makes an assertion about an individual event, not about a collection of events. Of course eminent physicists will be found to assert that "the probability concept is in a sense subjective" (Feynman *et al.*, 1963, p. 6–7). However, they fail to justify this claim. Moreover, when computing or measuring probabilities they do not regard their own results as items of (or based on) uncertain knowledge. There need be no more uncertainty about a probability than about a duration or a population count.

Let us now examine the main alternative philosophies of probability: the logical, subjectivist (personalist), and empiricist (or frequency) views. According to the former, probability is a certain relation between propositions: it explicates the notion of confirmation of a hypothesis by the empirical evidence relevant to it. This relation is supposed to be logical, hence independent of fact; consequently the theory built around it, namely inductive logic, is pronounced analytic (Carnap 1950) – a tragic dénouement for a story that originated in the empiricist praise of induction as the method of science.

The root trouble with the logical interpretation of probability is that there *are* no objective procedures for assigning probabilities to propositions, in particular to probabilistic hypotheses such as probability distributions. In science and technology such assignments are made either on the strength of measurements or of hypothetical random mechanisms, such as blind shuffling and genic mutation. But of course in these probabilistic models it is facts (states or events), not propositions, that are assigned probabilities. Moreover, there is nothing a priori about such assignments: they are all supposed to be corrigible in the light of new empirical information. In sum, the logical interpretation does not fit science or technology. It is also useless in pure mathematics, where the domain A of a probability measure is a family of abstract sets. To be sure these can be interpreted as propositions, since the latter satisfy formally the laws of the algebra of sets. But by forcing this particular interpretation one loses the great generality (hence portability) of the probability calculus. In short, the logical interpretation of probability fits neither pure nor applied mathematics. It is but a philosophical mirage – and a declining industry.

(It might be thought that the probability of a proposition can be interpreted in turn as the probability of the fact referred to by the proposition, so that the logical view would be suitable for applied mathematics after all. If this were so, why not interpret the arguments of a probability function as facts to begin with? Besides, the interpretation of the probability of a proposition as the probability of the fact represented by the proposition is possible only in the case of individual propositions. If h is a universal hypothesis, then '$P(h)$' cannot be interpreted in terms of facts because there are no general facts.)

Understandably the vast majority of the applications of probability in philosophy assume the logical interpretation and, particularly, the hypothesis that propositions can be assigned probabilities. Indeed philosophers of the most diverse kinds – such as Reichenbach, Carnap, Popper, Hin-

tikka, and Good, to mention only a few – have attempted to give probabilistic elucidations (usually reductions) of concepts as diverse as those of certainty, credibility, information, truth, content (or lack of it), confirmation, simplicity, and others. Since such theories never get down to numbers, except in the cases of tautologies and contradictions, they accomplish nothing even though they look exact. Another mistake some philosophers have incurred is holding that there is such thing as "probabilistic inference", which would be exactified and systematized by the probability calculus. Actually every valid reasoning with probabilities is an instance of the inference rules in the predicate calculus: the occurrence of probabilities in the premises makes no difference to the way the conclusions follow. In short, the theory of probability is not a generalization of deductive logic.

The *subjectivist* (or personalist) interpretation construes every probability value $P(x)$ as a measure of the strength of someone's belief in x, or as the accuracy or certainty of his information about x. (See de Finetti 1972, Jeffreys 1957, Savage 1972.) This view was historically the first and it is still very popular because it harmonizes with classical determinism, which has become part and parcel of the vulgar world view. In fact if the universe is deemed to be strictly deterministic (in the narrow sense), probability is resorted to only because of our ignorance of details and of the ultimate causes: God would have no use for probability. The paragon was the classical kinetic theory of gases: here the laws are causal, but probability is used allegedly because of our ignorance of the initial positions and velocities of the atoms or molecules. (This is not true: the theory does not say anything about such ignorance. Instead, it contains the hypothesis that the initial positions and velocities are distributed at random.) There are a number of objections to the subjectivist view, any one of which suffices to ruin it.

A first objection, of a mathematical nature, is that the formula "$P(x) = y$" makes no room for a subject u and the circumstances v under which u estimates his degree of belief in x, under v, as y. In other words, the elementary statements of probability theory are of the form "$P(x) = y$", not "$P(x, u, v) = y$". Such additional variables are supernumerary, and yet they would have to be introduced in order to account for the fact that different subjects assign different credibilities to one and the same item, as well as for the fact that one and the same subject is likely to change his beliefs not just in the light of fresh information but also as a result of sheer changes of mood – not to speak of spiritual crises altering a person's whole world view. In sum, the subjectivist or personalist interpretation of probability is adventitious, in the sense that it does not match the structure of the mathematical concept.

Second, there is empirical evidence against the thesis that our beliefs are actually so rational that they satisfy the probability calculus (or any other theory). For example, most of us hold pairs of beliefs that, on closer inspection, turn out to be mutually contradictory, so that they ought to be assigned nil credence. Another example: most of us do not believe in objective chance, in particular in mere coincidence (or statistical independence). Hence we tend to overrate the probability of the conjunction of independent events (Bar Hillel 1979).

(The subjectivist can circumvent this objection by claiming that the "calculus of beliefs" is a normative theory not a descriptive one. He may indeed hold that the theory *defines* the concept of rational belief, so that anyone whose beliefs do not conform to the theory departs from rationality rather than refuting the theory. Or he may claim that probability theory must be construed as a normative theory of rational behavior under risk, i.e. as decision theory (Wald 1950). The first move will save the interpretation from any ugly unfavorable empirical evidence, but by the same token it will deprive the interpretation of empirical support. And the second move fails for the following reasons. First, decision theory uses probabilities and therefore presupposes an independent theory of probability. Second, the probabilities occurring in standard decision theory are subjective, whereas a rational person is supposed to try and act always on objective probabilities. In real life people who maximize their expected utilities using subjective probabilities are said to indulge in wishful thinking instead of engaging in rational behavior. See Ch. 5, Sect. 5.2.)

Third, it is not true that probabilities are always assigned on purely subjective grounds, i.e. on no grounds whatever. In science and technology one assigns probabilities with the help of observation or measurement. (Even the so-called "principle of indifference", or hypothesis of equiprobability, often adopted in the absence of precise information, is not a principle but just a conjecture supposed to pass empirical tests.) For example, genetic models assign definite objective probabilities to certain genic changes, and the experimental biologist is in a position to test such theoretical values by contrasting them with observed frequencies. Of course we often make probability assignments, or conjecture probability distributions, in the face of insufficient information. But we do the same with many other "quantities" as well, i.e. we hypothesize or guesstimate probabilities just as we conjecture lengths or weights. (On the other hand nobody knows how to estimate the probability of either data or hypotheses. We do not even know what it *means* to say that such and such a formula has been

assigned this or that probability.) In sum, the subjectivist interpretation of probability is wrong. (See further objections in Marshak *et al.* 1975, Harper and Hooker, Eds., 1976, and Bunge 1976a, 1981*b*.)

(One way of bringing home the difference between objective indeterminacy and subjective uncertainty is to consider the Prisoner's Dilemma. Of three prisoners, named *A*, *B*, and *C*, only one is to be freed but neither of them knows which one it is to be. *A* asks the jailer to tell him whether *B* or *C* is to remain imprisoned. The jailer, believing that he is imparting no information about the chances of *A*, replies truthfully that *B* will stay in prison. If *A* is a subjectivist he will jump for joy, believing that his chances of being released have suddenly risen from $\frac{2}{3}$ to $\frac{1}{2}$ – just because the jailer blabbered. But if he is an objectivist he will realize that there are two possibilities: either all prisoners have already been sentenced, or only *B* has. If the former is the case, then *alea jacta est*: there is uncertainty but no probability. And if the latter is the case, then there are in turn two possibilities: either *A* or *C* will be judged, and one of them sentenced, or their fate will be left to chance – e.g. to the outcome of coin flipping. Being an objectivist, *A* knows that his *objective chances* cannot be influenced by what the jailer told him: only his own *uncertainty* has changed as a result. Moreover he realizes that, if he and *C* are to be judged, *neither can be assigned a probability* of being freed, for there are probabilities only where there is randomness. It is only if the judges decide to break the law, and commit the fate of *A* and *C* to the coin, that a probability will appear. And this probability is objective and it happens to coincide with the degree of certainty, namely $\frac{1}{2}$ – provided the coin is fair and the judges throw it fairly. The moral is clear: indeterminacy differs from uncertainty, so no prisoner should take consolation in personalist probability.)

Empiricists tend to believe that the correct alternative to subjective probability is frequency. There have even been attempts to define probability as a sort of limit of the relative frequency as the population size increases (Venn 1888, von Mises 1972, Reichenbach 1949). These attempts have been shown to be mathematically incorrect (Ville 1939, Fréchet 1946). Yet, if not the definition, at least the interpretation of probabilities as frequencies is widespread among scientists and technologists. My objections to this interpretation are as follows.

Firstly, probability and frequency, though related, are different concepts. For one thing, whereas the former is theoretical, the latter is empirical. For another, they have different mathematical structures. In fact, whereas a probability function is defined on an abstract probability space *A*, a fre-

quency function is defined, for every sampling procedure, on the power set of a finite subset of A, namely the collection of actually observed events (Bunge 1973a, Ch. 4, Sect. 2.5). Consequently a probability statement does not have exactly the same reference class as the corresponding frequency statement. Whereas the former refers usually to an *individual* (though possibly complex) fact, the latter is about a whole set of facts chosen in accordance with certain sampling techniques. (In other words frequencies, like averages and standard deviations, are *collective* properties not individual ones.) For example, one speaks of the frequency (not the probability) with which one's telephone is heard (by someone) to ring over a certain time interval, thus referring to an entire collection of facts.

Secondly, probabilities and frequencies seldom coincide numerically for, whereas the former are independent of the number of trials (or the populations size), frequencies fluctuate with it. Moreover, the two are clearly distinguished in a number of theorems, particularly in the laws of large numbers. For example, in a binary sequence of n independent trials (e.g. coin flippings), the (meta)probability that frequency and probability coincide numerically increases with n. This classical theorem cannot even be *stated* if probabilities are equated (via definition or interpretation) with limiting frequencies. (Actually the frequency occurring in this theorem is a theoretical concept, namely the average probability, which can be construed as the expected frequency.)

Thirdly, whereas every frequency is the frequency of actual observations of facts of some kind, a probability may be interpreted as the quantitation of a potentiality yet to be actualized. (Recall Vol. 3, Ch. 4, Sect. 4.) Consequently equating probabilities with frequencies (via definition or interpretation) involves rejecting real (objective) possibility – i.e. adopting an actualist ontology. (In turn this involves forsaking the understanding of the probabilistic models in science and technology, which take objective potentiality and randomness in earnest.) The equation of probability and frequency involves also restricting the domain of applicability of the probability calculus to observable phenomena, which constitute only a small subset of the collection of facts. Finally, the equation involves employing probabilities only posthumously, after all the observations have been made and sifted – never for predictive purposes.

The correct procedure with regard to the relations between probability and frequency is not to equate them but to clarify their differences and relations. In particular, frequency is among the estimators or indicators of probability. (It is not the only one. Recall that in physics probabilities are

often checked or estimated via other "quantities", such as spectral line intensities, temperatures, and scattering cross sections.) In short, there are often frequency *estimates* of probability, but the frequency interpretation of probability is mistaken.

We are left then with only two legitimate probability concepts: those of pure and applied probability, i.e. those of (normed) measure and tendency or propensity, respectively. The other three concepts – the logical, subjectivist, and frequency ones – are illegitimate. The logical interpretation is mathematically correct but far too restrictive and it has no application to science or technology: it is idle. And the other two interpretations are mathematically objectionable, unsuitable for science and technology, and philosophically implausible – at least with respect to the philosophical system that is being expounded in this *Treatise*.

The moral of this story is that pure and applied mathematics are clearly distinct, yet they must not be separated. Each of them has its own goals, which it can attain only by interacting vigorously with the other and by avoiding intercourse with obsolete philosophies.

5. Foundations and Philosophy

5.1. *Foundation of Mathematics*

Mathematics has foundations, though they are neither permanent nor above doubt. (See Hilbert and Bernays 1968, 1970, Wilder 1952, Beth 1959, Stoll 1963, or Hatcher 1982.) At any given stage in the history of mathematics, its foundations consist in the most general concepts, principles and methods of mathematics: it is the union of logic (*lato sensu*) and metamathematics. To be sure the odd philosopher – e.g. Wittgenstein (1978) or Lakatos (1978) – has held that mathematics stands in no need of foundations. But mathematicians are aware of the existence of foundations if only because the subject occurs together with logic at the beginning of every issue of *Mathematical Reviews*. However, most mathematicians work in areas so far removed from foundations that they need not care about the latter. In fact their work is largely foundation-free, or compatible with alternative foundations. Therefore it is unlikely to be shaken by any revolutions in foundations. Yet it may be considerably enriched, or at least illuminated, by foundational results.

The foundations of mathematics, or of any other science, has several motivations. One of them is *philosophical*: the wish to harmonize

foundations with some philosophical system, such as rationalism, intuitionism, or empiricism. Another is *psychological*: the wish to attain ultimate certainty. The logicists sought the latter in the reduction of all mathematical objects to logical ones; the formalists, in proving the consistency of every mathematical theory; the intuitionists, in relying on a handful of allegedly infallible intuitions. All three moves failed, and yet foundational work went on: we have come to realize that "A Foundational system serves not so much to prop up the house of mathematics as to clarify the principles and methods by which the house was built in the first place". (Goldblatt 1979 p. 14). Moreover we have come to expect that the foundations of mathematics will continue to shift and even branch out as a result of research into them. Thus a distinguished foundational worker notes that "the basic theory of elementary toposes [...] seems to be almost completely worked out [... Doubtless there are minor points to be cleared up,] but the foundations of the subject do appear to be pretty stable. This is of course a bad thing: it is vital to the health of a subject as basic as topos theory that its fundamental tenets should be the subject of continual review and improvement, and I am uncomfortably aware that by writing this book I have contributed largely to the concreting-over of these foundations" (Johnston 1977 p. xvi).

A third motivation for foundational work is the yearning for *simplicity*, which would be satisfied by reducing the whole of mathematics to a single theory (e.g. logic or arithmetic). This too has been and continues to be a powerful rationale even though it is but a mirage, as shown by the progressive complication of the field. Finally, a fourth motivating force is the wish to *systematize* the various chapters of mathematics: to put them together into a single vast system. This is perhaps the most legitimate of the four motivations, because it is realizable and it has yielded abundant fruit, such as the formal systems underlying their various models, and the functors relating the various systems. Besides, it has the advantage of promoting cooperation among specialists and facilitating the understanding of mathematics. In sum, there is no question that mathematics has foundations, albeit shifting ones that are unpopular subjects of study even among mathematicians.

Those few mathematicians who do care about foundations are either satisfied or dissatisfied with standard logic and metamathematics. The former, who constitute the vast majority, believe that standard (ordinary, "classical") mathematics is basically correct and has never been so prosperous. Their critics worry about the standard logical foundations of mathematics, to the point of speaking occasionally of "a crisis in contemporary

mathematics [...] *due to our neglect of philosophical issues*" (Bishop 1975 p. 507). Now, it is true that the foundations and the philosophy of mathematics are intimately related. (The same holds for any other science.) In fact the type of foundations one favors is usually motivated and justified by philosophical theses concerning the nature of mathematical objects and the way we get to know them. For example, logicism has been wed to Platonism, formalism to nominalism, and constructivism to intuitionism.

However, it is possible and convenient to separate foundational from philosophical issues, in view of the fact that the above mentioned historical associations are causal rather than logical, as shown by their considerable weakening in recent years. Thus it is possible to adopt intuitionistic logic without admitting its original philosophical motivations or even without adopting constructivism. And, although constructivism was initially associated with Kantian intuitionism, and even with operationism (see Bunge 1962a), most contemporary constructivists seem to be indifferent to philosophical matters. They do not seem to believe that "intuitionistic mathematics is pointless without the philosophical motivation underlying it" (Dummett 1977 p. viii) – which is fortunate because that motivation is controversial to say the least. For those mathematicians constructivism is an interesting research strategy free from the risks of alternative policies. Many constructivists are just curious to find out how much "classical" mathematics can be rebuilt in a constructive fashion ("constructivized"), and what new vistas the constructivist approach may open. Likewise one may adopt a logicist strategy, or a formalist one, or even a combination of the two, without admitting any of the philosophies traditionally linked to them. In short here, as in every other research field, current work can be dissociated from its historical roots. We may then, up to a certain point, and breaking the tradition, uncouple foundational matters from philosophical ones.

Now, there are two classes of foundational strategy or policy: *restrictive* and *non-restrictive*. The latter demand only that logical conditions be obeyed, in particular exactness, systemicity, and non-contradiction: aside from these minimal constraints the mathematician should feel to invent whatever he feels like. This is of course the strategy tacitly adopted by "classical" mathematics and the one explicitly advocated by logicism. The restrictive strategies, on the other hand, set additional constraints on mathematical inventiveness. Thus formalists and constructivists alike reject actual infinity and admit only finitistic methods. The main difference between them, aside from their motivation, is that the constructivist demands in addition that all concepts and proofs be constructive at least hypothetically, i.e. in principle.

We proceed to examine briefly these various foundational strategies without regard for the philosophies supposed to buttress (or discredit) them. (By dismissing any of them on purely philosophical grounds we would risk missing important mathematical contributions.) Nor shall we waste much time on propagandistic claims such as "Policy X is more *natural* than policy Y", or "Concepts of type X are more *meaningful* than concepts of type Y", because their proponents do not bother to tell us what "natural" and "meaningful" mean to them. (In particular, intuitionists are remarkably vague with regard to the words 'intuitive' and 'meaning', which they use very often. Thus Dummett (1977), an eminent philosopher of intuitionistic mathematics, offers no theory of meaning.)

It is well known that the classical schools in the foundations of mathematics are logicism, formalism, and intuitionism. (See Wilder 1952, Beth 1959, Hatcher 1982.) These schools were formed roughly between 1890 and 1910, but they have mellowed and even cross-fertilized since then, to the point where there are hardly any pure logicists, formalists, or intuitionists left. Let us review them quickly.

Logicism, advocated mainly by Frege and Russell, and favored by most logicians and exact philosophers, comes in two strengths. The *weak* thesis is that every mathematical theory contains logic – or, equivalently, that every mathematical theory has a logical foundation. (I.e., if $\mathscr{T} = \langle F, \vdash \rangle$ is a mathematical theory, the set F of its formulas contains the totality of tautologies of some system of logic.) This thesis is controversial except to Brouwer and his faithful, who claim that mathematics precedes logic. It is also philosophically rather uninteresting.

The *strong* logicist thesis states not only that mathematics is based on (i.e. presupposes or contains) logic, but also that it is fully *reducible* to it: that every mathematical concept is definable in logical terms, and every mathematical formula is derivable from logical formulas. (Recall the definition of full or strong theory reduction: Vol. 6, Ch. 10, Sect. 3.1.) However, this thesis can be construed in two different ways, according as logic is conceived in a strict (narrow) or in a wide sense. According to the former construal, mathematics is reducible to elementary logic, i.e. to the first order predicate calculus. Clearly, this version of logicism is false, as shown by the following two elementary counterexamples. The abstract relation of partial order is not reducible to (definable in terms of) any of the concepts occurring in first order logic; the same holds for the abstract operation of concatenation, which occurs in algebra. In short, mathematics is not reducible to first order logic; in particular it is not a heap of tautologies.

We must therefore construe the logicist thesis in its lax sense, namely as the contention that mathematics is reducible to logic *lato sensu* – i.e. the predicate calculus of the first and higher orders, model theory, set theory, and possibly further universal theories (such as category theory) as well. If understood in this wide sense, it must be admitted that, like many another reductionist strategy, logicism has had some stunning successes. An early victory was the reduction of arithmetic to the predicate calculus augmented with set theory. But, like every other radical reductionist program, logicism has been shown to have limitations. For one thing, since there are alternative set theories (every one of which defines implicitly its own membership relation), there can be no *unique* reconstruction of mathematics on the basic of logic *lato sensu*. So, the original program of logicism should be reformulated in a more realistic way, namely this: "Reduce as far as possible but recognize the limits to reduction when you encounter them. When you do encounter such limits you are facing new, emergent constructs, related to but not reducible to the previously introduced ones". Even cut to this size, logicism is invaluable for its insistence on exactness and on the need for laying bare the logical basis and the logical structure of mathematical theories.

Logicism has often been misunderstood by philosophical friends and foes. One such misunderstanding is equating it with the thesis that, "in principle", mathematicians need know only the predicate calculus and set theory to work, say, in number theory. Actually, even when doing the simplest sums we had better forget the reduction to logic, because the bridging or defining formulas (e.g. $\ulcorner 1 =_{df} \{\varnothing\} \urcorner$) can get so cumbersome, that the arithmetical propositions become unmanageably long and unperspicuous. Consequently "we continue to do arithmetic as before only with the awareness that there is a sense in which our proofs and calculations could be translated into set theory" (Wang 1966).

A second popular misunderstanding of logicism is revealed by those critics who hold the logical reconstruction of a piece of mathematics to be circular for presupposing (in the epistemological sense) a prior intuitive knowledge of that piece (Casanova 1947, Steiner 1975). This objection misses the point of any rational reconstruction, which is not to build new theories, let alone to teach them, but to bring order and possibly also consistency into a somewhat messy body of knowledge that has grown naturally in a zig-zagging historical process. Logicism is not to be understood as the thesis that logic oozes mathematics, but that mathematics can be reconstructed out of logical constructs alone. That such reconstruction is not always possible, is another matter.

The next foundational strategy to be examined is *formalism* (Hilbert 1935, Hilbert and Bernays 1968, 1970, Curry 1951, Robinson 1965). Formalism too, has changed over the years, though perhaps to a lesser extent than logicism, and it has been no less misunderstood than logicism. For example, Davis and Hersh (1981), following Lakatos (1976), have attacked formalism for failing to provide a faithful account of the actual process of mathematical research. The latter, as we know, is no less zigzagging and subject to error and uncertainty than any investigation in natural science. But the point of formalism was not to account for the psychology and history of mathematics: it was to reconstruct in a rigorous way the outcome of a rather unruly process. Moreover this clean copy had to meet the objections that Brouwer had raised against the logicist reconstruction, and it had to save the bulk of mathematics, which had been threatened by Brouwer. The rigorous reconstruction was intended to culminate a process of spontaneous growth, not to replace it, let alone to describe it.

The main technical features of formalism are abstraction, finitism, axiomatics, and metamathematics or proof theory. (We regard the nominalist thesis, that the basic mathematical objects are marks or signs – e.g. numerals instead of numbers – as a philosophical thesis. Accordingly we shall deal with it in Sect. 5.2.) Let us begin with abstraction. Although there had been abstract theories before – e.g. Boolean algebra and group theory – Hilbert stressed their importance as well as the radical difference between such theories and those where every symbol is interpreted in a "concrete" (possibly also familiar) fashion. (Recall what was said about model theory in Sect. 3.1.) Hilbert succeeded even in digging up the abstract theory underlying elementary geometry – which won him the hatred of intuitionists.

A second feature of formalism was finitism: the attempt to complete the process of eliminating infinity from mathematics – a process started by Weierstrass when he replaced intuitive considerations about limits, as well as "infinitely small" and "infinitely large" quantities, with reasonings about finite deltas and epsilons. To be sure Hilbert dit not reject actual infinity but regarded it as a fiction, and correspondingly all talk about actual infinity as a *façon de parler* – as if finite sets were any less fictitious. (See Hilbert 1925.)

A third feature of formalism was the program of axiomatizing all mathematical (and also physical) theories. The aim of axiomatics is to seize on the basic ideas (primitive concepts and postulates) and arrange them in an orderly and consistent manner (See Bunge 1973a, Chs. 7 and 8.) This can only be attained by disregarding historical accidents and pedagogical devices. (Inevitably, this feature of formalism was to irritate the lovers of

confusion as well as of the people unaccustumed to viewing theories as wholes.) A fourth characteristic of formalism was the systematic metamathematical study of theories and, in particular, the attempt to prove their consistency by finitist methods.

All four proposals came eventually under fire. The high regard for abstraction, joined with the nominalist thesis that the basic objects of mathematics are marks on paper rather than concepts, was taken for the ludic doctrine according to which mathematics is "a sort of game, played with meaningless marks on paper rather like noughts and crosses" (Ramsey 1931 p. 68). Had Hilbert been a consistent nominalist he would have espoused the ludic doctrine, for this follows from identifying the objects of mathematics with symbols, and its laws with conventional rules. Fortunately he was not consistent in this regard, as shown by the kind of problems he grappled with or proposed to others: they were anything but games. Much the same can be said about the early Whitehead, who had defined algebra "as an independent science dealing with the relations of certain marks conditioned by the observance of certain conventional laws" (1898 p. 11). In any case the view of mathematics as a game did not last except among some philosophers such as Wittgenstein (1978). With the rise of semantics people realized that, unlike mathematical theories, games have no metatheories and do not involve a notion of truth (Fraïssé 1982).

The most vulnerable thesis of formalism turned out to be the one about the scope of formalization. It suffered a severe blow when Gödel (1931) proved that every consistent theory containing arithmetic is necessarily incomplete. However, in my view this setback did not dash Hilbert's program of axiomatizing all mathematical theories and proving their consistency. It only *restricted* the scope of axiomatics and killed the hope of proving the consistency of mathematical theories with the sole help of *finitist* methods. The axiomatic format, in particular the axiomatic definition of new concepts, has become standard not only in the foundations of mathematics but in all of abstract mathematics, particularly in the work of the Bourbaki team – which, incidentally, embraced the formalist strategy (Dieudonné 1981) though without caring for foundational studies. Axiomatics has also been used in the foundations of science (see e.g. Bunge 1967c, 1973a and Suppes 1969) as well as in philosophy (see the first four volumes of this *Treatise*). Besides, the line of metamathematical investigation initiated by Hilbert and Peano has proliferated, though with two curious features. The first is that it has flourished outside the main formalist school, the Bourbaki

team. The second is that most of its results have been negative, i.e. of the type "Theory x is incomplete", and "Theory y is non-categorical". In short formalism, just like logicism, is far from dead. In a weakened form and dissociated from nominalistic philosophy, formalism has become part of the mainstream of the foundations of mathematics.

We now leave the mainstream, though by no means the vanguard, to have a look at intuitionism. Brouwer, the founder of the school, championed both radical constructivism and Kantian intuitionism, as well as some mystical ideas that have been set aside by his followers. It may be said that his slogan was *No existence without construction, and no construction without intuition.* Brouwer crusaded against "classical" or standard mathematics for being full of non-constructive concepts, such as that of a continuum, and non-constructive proofs (by reduction to absurdity). In particular he did not accept the excluded middle and he rejected actual infinity as well as the axiom of choice. He also denied the priority of logic. And he failed to recognize that constructivity, surveyability and intuitability are not absolute but come in degrees.

Intuitionism has changed considerably since Brouwer's day: it is anything but a small homogeneous sect. First Heyting formalized intuitionistic logic, a feat seen by many as a suicidal concession to formalism. Later on a number of mathematicians embraced constructivism while paying no attention to the philosophical underpinnings of intuitionism. (This convergence was unavoidable given that formalism too is constructivist: see Curry, 1951 and Markow, 1975.) More recently, the computer revolution has reinforced the constructivist camp with a huge crowd of computer scientists convinced that mathematics is a collection of algorithms. And lately a number of mathematicians have tackled abstract mathematics – a contradiction in terms to a true intuitionist – on the basis of intuitionistic logic though without admitting constructivism. So, 'intuitionism' has come to mean different things to different people.

For half a century intuitionistic mathematics yielded only a meagre crop. Consequently adopting it involved sacrificing the bulk of mathematics on the altar of a cruel god fired by a dubious philosophy. Even Heyting (1957) admitted that such sacrifice was "an inevitable consequence of our standpoint". This situation altered radically when Bishop (1967) and a few others succeeded in "constructivizing" a large portion of analysis. (See Stolzenberg 1970 for a paean and Goodman 1983 for a cold shower.) Since then intuitionistic mathematics, or what passes for such, has been thriving. (See Troelstra and Van Dalen, Eds. 1982.)

However, as noted before, not all constructivists are intuitionists: indeed some are formalists. And not all those who call themselves 'intuitionists' are constructivists: some accept intuitionistic logic but make use of nonconstructive principles and methods. Moreover the constructivist principle comes in two strengths: a moderate one and a radical one. *Moderate constructivism* is the thesis that every concept and every proof are to be constructive: that we must always show an explicit routine for constructing our concepts or proofs – hence we must abstain from asserting the existence of anything that we do not know how to construct. For example, we can talk about sets only provided we define them; and we can assert that an equation has a solution only if we know how to find at least an approximate solution to it. In short, *to be is to be constructible*.

Radical constructivism, proclaimed by Brouwer and revived by Bishop, goes much further: it demands that every construction have a "computational meaning" and, in general, that all of mathematics be assigned a "numerical meaning". This means that, however high on the scale of abstraction we may be working, we must always be able to "get down to numbers" – meaning the positive integers. In other words, mathematics should be based on the natural numbers rather than on undefined predicates, abstract sets, or morphisms. *To be is to be a natural number or to be constructible out of natural numbers.*

Radical constructivism has been at work in analysis. Outside analysis, particularly in logic, algebra, and topology, most of the mathematicians who call themselves 'intuitionists' adopt the moderate version of constructivism or just are not constructivists and accept only intuitionistic logic. The reason for this is plain, namely that radical constructivism makes no room for abstract objects, which are the ones occurring in abstract algebra and general topology. (Abstract objects are the *bête noire* of all non-rationalists.)

Since constructivism is gaining ground, we had better take a closer look at it. The hub of constructivism, whether moderate or radical, is the concept of existence. In "classical" (standard) mathematics, exactness and consistency guarantee formal existence: recall Sect. 2.1. In particular, by the completeness theorem of first order standard logic, every consistent theory based on the predicate calculus has at least one model: so, the existence of the objects in any such model is warranted even before we proceed to define them. The constructivist recognizes no such birthright: he admits only existence by construction: he wants to see the baby before christening it. In this he resembles the Greek geometers, who demanded that every geometrical figure and proof use only rule and compass. (This excluded, of course,

any reasonings on infinite lines or planes.) He also resembles the operationist, who claims that to exist is to be measurable. A few elementary examples will suffice to appreciate how straight and narrow the constructivist path is.

Example 1 In standard set theory, and in the set-theoretic reconstruction of arithmetic, the most important set is the empty set. Constructivism declares this set to be nonexistent because one cannot construct nothingness. (Remember the times when people doubted that 0, or even 1, were numbers?) *Example 2* In standard mathematics one may prove a theorem (e.g. the assertion that there is a single empty set) by showing that its denial leads to contradiction. This mode of proof (by *reductio ad absurdum*) is not available to the constructivist, who must prove either a proposition or its negation. (Moreover constructivists try to avoid negation: they are literally positivists.) *Example 3* In "classical" analysis one proves the so-called intermediate value theorem: A continuous real function f defined on a closed interval $[a,b]$, such that f is negative at a and positive at b, vanishes somewhere in between. I.e. *there is* a point $a < \xi < b$ for which $f(\xi) = 0$. Constructive analysis does not contain this theorem for it does not tell us how to compute ξ. (Recall Fig. 1.4 in Sect. 3.2.)

This last example is quite typical of the mutilating effects of the constructivist strategy. Not only most analysts but also all factual scientists are keen on the intermediate value property because they use it daily, albeit tacitly most of the time. In fact to go from one place to another we must pass through every intermediate point along our route – and we do so even when we do not know what points we are passing. (This refutes the contention that "the only reason mathematics is applicable, is because of its inherent constructive content" (Bishop, 1975).)

Nor is that the only sacrifice we are asked to make to comply with intuitionism. All the other existence formulas, among them the axiom of choice, are to be given up as well unless they can be coupled to constructive procedures (algorithms). This is not all: orthodox intuitionists must also believe Brouwer's proof that all real functions are continuous. That is, they cannot admit discontinuous functions such as the step function (unless approximated by a continuous function). Hence intuitionism cannot analyze such discontinuous physical processes as the collision of elastic bodies or the refraction of light waves. It is not true, then, that the whole of "classical" analysis has been, or even can eventually be, "constructivized". And, because of some of its limitations, it will never be able to be as useful to factual science and technology as standard analysis. Which is not sur-

prising taking into account that intuitionism is the companion of operationism.

The limitations of constructivism do not entail that it is an unacceptable strategy for theory construction. It *is* admissible provided its imperial wings be clipped. More precisely, constructivism is legitimate provided it tolerates nonconstructive concepts and methods side by side. In other words, we should acknowledge *two methods* of bringing mathematical constructs into (formal or conceptual) existence: postulation and construction. The former, or "classical", method demands only consistency. (If P is a predicate defined implicitly by a system of postulates, then P is non-contradictory iff the extension of P is non-empty, i.e. if there are objects with property P. In this case P may be said to exist conceptually *by fiat*. The scope of this method is somewhat limited because of Gödel's incompleteness theorem.) The constructive method is far more demanding and, therefore, psychologically reassuring: "The only way to show that an object exists [constructively!] is to give a finite routine for finding it" (Bishop 1967 p. 8). There is no justification, other than philosophical bias, for excluding either of these methods. We should make full use of the unique freedom we enjoy in mathematics, letting the factual scientists and technologies worry about the constraints inherent in the real world.

Now a word on an extreme version of constructivism, namely *recursivism* (e.g. Grzegorczyk 1959). The slogan of recursivism is that analysis should boil down to the theory of recursive functions. (A recursive function of one variable takes natural numbers into natural numbers.) Recursion theory was initially invented to exactify the rather hazy notion of computability, and most mathematicians regard it as a smallish chapter of analysis. (It is sometimes included in logic because of its importance in metamathematics, which is like regarding analysis as a branch of physics because of its central role in this science.) The current vogue of the theory is due to its importance in computer science. If computers could have a philosophy they would be recursivists and nominalists.

Recursivism demands an intolerable mutilation of analysis. In fact the most general and interesting functions are the real functions, and particularly the continuous ones, which lie beyond the scope of recursion theory. The latter analyzes the way we *compute* values of functions to any desired degree of accuracy, but it cannot even *define* such elementary functions as the linear and the logarithmic functions. Hence recursion theory may be regarded as ancillary to "classical" analysis, never as a substitute for it. Therefore recursivism, unlike the less radical versions of constructivism, is not a possible foundational strategy. Its inability to recapture a substantial

part of "classical" analysis should be taken as a warning against the attempt to reduce all mathematical objects to the positive integers.

In sum, there are alternative foundational strategies which deserve being tried out. Most contemporary mathematicians have tacitly adopted a combination of logicism and formalism. And, although most mathematicians do not care for the strictures of intuitionism, they are all happy when a "classical" result is reproduced by constructive methods. So, all three major foundational strategies have their place in contemporary mathematics, and neither dominates absolutely. For example, new concepts are sometimes introduced constructively in a step by step fashion, as demanded by both logicists and constructivists; and other concepts are introduced by postulate systems (i.e. creatively), as recommended by the formalists. And, whereas some proofs are direct, most are indirect (i.e. they use the excluded middle). middle).

The adoption of a single foundational strategy at this time would impoverish pure mathematics and, consequently, it would diminish its power as a tool for building factual theories. Foundational monism would also jeopardize our understanding of the nature of mathematics, for each of the major foundational strategies does capture one aspect of mathematics. Thus logicists and formalists stress the deductive side of mathematics, whereas intuitionists stress its nondeductive side. Indeed, where the former say that every theorem follows from a set of postulates and definitions, intuitionists claim that it is the other way round: that we fashion the postulates and definitions so as to get the results we want or need. Actually there is no incompatibility between the two theses: the former concerns finished mathematical theories, whereas the latter is about mathematics in the making. (In other words, whereas logicists and formalists focus on the deductive structure of mathematics, intuitionists stress its heuristics.) Hence the two views on the nature of mathematics are mutually complementary rather than incompatible. Adopting one of them to the exclusion of the other is therefore as unwise as opting between biological systematics and evolutionary biology, or between sociology and history. Every major foundational strategy has its merits and shortcomings, and every one of them sheds some light on certain aspects and chapters of mathematics while leaving others in the dark. Consequently we should advocate *foundational pluralism* at least for the time being.

The thesis that alternative foundational strategies are admissible generalizes the 19th century finding that there are alternative geometries, as well as the more recent finding that there are alternative set theories (every one

of which exactifies a different notion of membership). This does not entail that all foundational strategies are worth the same, so that the choice among them is purely a matter of taste. As a matter of fact some strategies yield more general and deeper foundations than others. Thus the reduction of mathematics to category theory results in a deeper, more vast building than the reduction to (a) set theory, which in turn goes wider and deeper than the reduction to arithmetic. So, if the desideratum is generality and depth, we know which strategy to choose. But one may have alternative desiderata. The choice of a foundational strategy resembles that of an architectural style. It is partly a matter of function: what do we want the building (or the theory) for? It is also partly a matter of resources: what materials and procedures (assumptions and methods) are we willing to employ? Hence it is also partly a matter of budget: what price are we prepared to pay? Finally it is also partly a matter of taste (philosophy). There is nothing wrong with admitting a plurality of styles in either mathematics or buildings, provided we do not mix them in one and the same construction.

As a matter of historical record each foundational strategy has been partly motivated by a given philosophy: logicism by idealism (in particular Platonism), formalism by nominalism, and mathematical intuitionism by philosophical intuitionism. To be sure, neither has been totally faithful to its sources. In particular the mathematical intuitionist's intuition is supposed to be the basis for a laborious intellectual construction, whereas Kant's pure intuition was supposed to deliver its object directly. Still, the ties are there.

Therefore, if we wish to uphold a pluralistic foundational policy ("philosophy") we must cut the traditional ties of foundations with philosophy: we must build a new philosophy of mathematics compatible with foundational pluralism. This we have done in outline in the previous sections. Let us now complete that sketch and compare it with alternative philosophies of mathematics.

5.2. *Philosophies of Mathematics*

A philosophy of mathematics should propose well-founded answers to such questions as: What is mathematics and how does it differ from the other sciences?, What is the nature of mathematical objects and how do they differ from material objects?, How do mathematical objects exist?, Does mathematics have any ontological presuppositions?, Is mathematics a priori, a posteriori, or both? What is mathematical truth?, What is mathematical proof?, How does mathematics relate to elementary logic and to semantics?, and How can mathematics, which is intemporal, cope with

reality, which is changing? There are, of course, many other problems that a philosophy of mathematics should be able to tackle. On the other hand a philosophy of mathematics need not deal with empirical problems that are best approached by factual disciplines such as the psychology and the sociology of mathematics – e.g. How do we fashion or learn mathematical ideas, and How do mathematical communities emerge, evolve, and decay? (Typically, empiricists, intuitionists and dialectical materialists refuse to draw this distinction between conceptual and empirical questions about mathematics. This distinction is necessary, even though a suitable understanding of mathematics as a whole calls for both conceptual and empirical inquiries. We must discern between distinction and separation, and insist on the former while rejecting the latter.)

A fair number of philosophies of mathematics have evolved over twenty five centuries, and nearly all of them are still alive. (See Brunschvicg 1929, and Benacerraf and Putnam Eds. 1964.) This is due not only to dogmatism but also to the fact that every one of them accounts satisfactorily for *some* aspect or other of mathematics while leaving the other aspects in the dark. This can be understood upon reflecting that any philosophy of mathematics may have, but as a matter of fact seldom has, all of the following components:

(i) *ontology*: questions about the ontological status of mathematical objects;

(ii) *semantics*: matters of sense, reference, and truth in mathematics;

(iii) *epistemology*: questions about the nature and sources of mathematical knowledge;

(iv) *methodology*: matters of justification (in particular proof) and application.

Nearly every philosophy of mathematics has focused on a couple of these components, presumably those where it had a positive contribution to make. The remaining components were neglected, either because overlooked or because they proved embarrassing to the general philosophy of which the given philosophy of mathematics was but a branch or an application. In contradistinction to such partial philosophies of mathematics, we need one capable of accounting for all four components above, as well as being compatible with the general philosophical ideas formulated in the previous volumes of this Treatise. But, since we have granted beforehand that a number of philosophies of mathematics have grasped correctly some traits of their object, let us review them quickly and then evaluate them.

The major classical philosophies of mathematics are Platonism (a variety

of objective idealism), nominalism (a variety of materialism), intuitionism (a variety of subjective idealism), and empiricism (in particular pragmatism). Their main theses are, schematically, as follows.

PLATONISM

Ontology: All ideas are self-existing: we only grasp some of them (imperfectly).

Semantics: Every non-contradictory construct is meaningful; truth, whether formal or factual, is correspondence with objective (ideal or material) reality.

Epistemology: The soul may remember some of the ideas it was originally exposed to; in any case we recall or discover but cannot invent any ideas.

Methodology: The axioms must be self-evident, and the theorems must follow deductively from them; as for applications, they consist in discovering the mathematical (ideal) properties of things.

NOMINALISM

Ontology: The objects of mathematics are symbols (of nothing), e.g. marks on paper: there are no "abstract entities".

Semantics: Nil.

Epistemology: Doing mathematics is playing a game with symbols according to definite rules. Mathematics has no subject matter.

Methodology: The pinnacle of mathematical research is the construction of formal axiom systems unattached to any particular interpretation. Only deductive proofs in consistent formal theories can be fully rigorous.

INTUITIONISM

Ontology: Mathematical ideas are mental objects and personal creations.

Semantics: Only intuitive mathematical ideas – in particular those which are ultimately reducible to the *Urintuition* of the sequence of natural numbers – are meaningful.

Epistemology: Mathematical knowledge is obtained by intellectual intuition rather than by sense experience or by pure reason. Whatever is counterintuitive (e.g. non-computable numbers and actual infinities) is not really known, hence it is no part of mathematics.

Methodology: Only constructive concepts and proofs are admissible.

EMPIRICISM

Ontology: Mathematical ideas are mental objects.

Semantics: Mathematical ideas mirror or summarize experience.

Epistemology: Mathematical knowledge is gotten in the same way as any other knowledge – in particular inductively. Mathematical axioms are nothing but pills of empirical wisdom.

Methodology: The ultimate test of mathematical ideas is human experience, however indirect – e.g. through the experimental test of scientific theories involving mathematics.

There are of course a few alternative philosophies of mathematics, but they are sketchy and have exerted no influence on mathematical research. The best known are those of Wittgenstein and Lakatos. Wittgenstein (1978) dealt mostly with "household mathematics" (Bernays 1976) – i.e. grammar school mathematics – and, tangentially, with some of the foundational problems investigated by Russell. His central interest with regard to mathematics was its psychology: matters of learning, understanding, inventing, and above all using elementary mathematical ideas. His entire philosophy of mathematics boils down to the pragmatist thesis – already found in Mach – that all mathematical propositions are *rules* (for computing or drawing), so that doing mathematics is a special case of following rules or playing games. This view is utterly inadequate if only because (*a*) it ignores that in mathematics all rules are based on laws, and (*b*) it overlooks the heart of mathematics, which is the building of mathematical systems (e.g. $\langle \mathbb{N}, 0, + \rangle$) and the proving of theorems about them. We shall therefore disregard Wittgenstein's opinions on mathematics.

As for Lakatos's ideas on mathematics (Lakatos 1976, 1978), they boil down to the following theses. Firstly, mathematical research is not essentially different from scientific research, for it too involves making conjectures and looking for counter-examples to them. Secondly, since one often starts with inexact concepts and one can make mistakes in proving theorems, one should adopt a fallibilist epistemology of mathematics. Thirdly, formalism does not depict faithfully the actual work of the mathematician, which involves nondeductive procedures. While all three theses are reasonable, neither of them is original, and they do not amount to a philosophy of mathematics. For one thing Lakatos had not clear ideas of his own about the nature of mathematical objects: he was more interested in the history than in the ontology or the semantics of mathematics. For another he exaggerated fallibilism to the point of skepticism, of denying that mathematics has any foundations, and of minimizing the role of logic – for all of which he earned the admiration of many a friend of sloppiness and inconsistency. So much for the marginal philosophies of mathematics. We turn now to the major classical schools.

Platonism is the philosophy associated with the logicist foundational strategy. (See e.g. Frege 1884, Gödel 1947, 1951, Wedberg 1955.) The best and shortest characterization of Platonism is due to Charles Hermite (of Hermite polynomials fame) in a letter to Stieltjes (of Stieltjes integral fame): "I believe that the numbers and functions of analysis are not the arbitrary product of our mind; I think that they exist outside ourselves with the same necessity as the things of objective reality, and that we meet, discover, or study them, just as the physicists, the chemists, and the zoologists" (art. "Realism", Lalande, Ed. 1938).

It is not hard to imagine the source of this philosophy and to explain its popularity. Trace a circle on sand: it will be imperfect compared with our idea of a circle, so you may have to retrace it or apologize for your clumsiness. Moreover your work will soon be wiped out by wind or water, whereas your mind will retain the idea of a perfect circle – an impersonal and universal idea. Ideas, then, and particularly the well defined ones of mathematics, seem to be perfect, endurable, and universal – unlike their material "embodiments", which are but imperfect, ephemeral, and circumstance-bound copies of the former. What can sound more self-evident, or better grounded in experience, than Plato's theory of Ideas (or Forms) and its application to mathematics?

No wonder that Platonism is the spontaneous philosophy of mathematicians: that most of them believe that they explore and eventually discover objects "existing independently of our definitions and constructions" (Gödel 1951 p. 137). Thus one mathematician asserts – without hinting at any possible evidence – that a mathematical structure "exists independently of any mind, and would exist even if there were no mind" (Goodman 1983 p. 65). And in a widely publicized interview, the Princeton professor William Thurston stated that "Theorems just kind of exist, you know, just like mountains do". In our view this is an intelligent mistake. It is a mistake because formal existence is radically different from material existence: recall Sect. 2.1. But it is intelligent because, as a matter of fact, the mathematician behaves in many regards *as if* constructs existed by themselves. He can do so because mathematical constructs, though human creations, do not bear the stamp of their creators: they are impersonal or intersubjective (though not objective).

In addition to accounting for the universality and apparent objectivity of mathematics, Platonism has been attributed virtues which either are not such or are shared by other philosophies. Its most obvious virtue is that, by proclaiming the logical autonomy of mathematics *vis-à-vis* all other fields of

knowledge, Platonism allows for the greatest freedom of mathematical research. This stands out in contrast to the limitationist policies of intuitionism and empiricism. However, to ensure research freedom we only need to demarcate formal from factual science the way we did in Sect. 1. In this fashion research freedom is protected at a far lower price, without having to buy the weird thesis of the independent existence of ideas, which contradicts physiological psychology.

A second virtue Platonism is often credited with is simplicity. But simplicity is more often an indicator of superficiality than truth. Mathematics calls for an ontology, a semantics, an epistemology and a methodology of its own, distinct from those of factual science or technology. For example, proving a theorem is in every respect quite different from testing a hypothesis in the laboratory. A third virtue Platonism is attributed is that it accounts for multiple independent discovery or invention: this would prove that ideas exist by themselves and are up for grabs. However, there is a scientific explanation for such coincidences: similar brains, trained similarly, and sharing a fund of knowledge, are likely to come up with similar solutions to the same problems.

Besides being less meritorious than often assumed, Platonism has a number of fatal flaws. Firstly, it makes no room for non-standard models of abstract theories and for non-standard logics – or at least it does not help us choose among the bewildering multiplicity of logics, all of which would coexist peacefully in the Realm of Ideas. Secondly, it makes no room for partial truths, which are the staple of approximation theory and factual science. Thirdly, and most important of all, there is no shred of evidence for the fantasy that there are ideas in themselves, i.e. separate from brain activity: this is just a dogma. (Recall Vol. 4, Ch. 4, or Vol. 5, Ch. 1.)

Despite all of the above-mentioned defects, most mathematicians behave like Platonists even while paying lip service to some other philosophy Thus Bourbaki, officially a formalist, believes in the "reality of mathematics" (Dieudonné 1970 p. 145). This is because Plato had grasped correctly the impersonality and universality of mathematics, as well as the immateriality and unchangeability of its objects, and because he allowed mathematicians utter freedom as long as they proceed rationally, in particular consistently. However, our own philosophy explains the same features of mathematics and grants the same creative freedom but, unlike Platonism, it is consistent with science, which does not recognize disembodied ideas.

Nominalism is the philosophy behind formalism. (See e.g. Leśniewski 1927–31, Chwistek 1932, 1949, Zinov'ev 1973, Field 1980.) It is a form of

vulgar materialism (*a*) for treating mathematical and physical objects in one breath (i.e. for refusing to distinguish conceptual from physical existence), (*b*) for asserting that there are general signs but no general, let alone abstract, ideas, and (*c*) for holding that a general sign does not designate a whole (such as a line) or a natural kind (such as a species) but denotes severally the individuals it points to. Since mathematics handles symbols (rather than concepts), and since symbols are conventional, mathematics turns out to be just as conventional as chess. Consequently the mathematical formulas are true by virtue of the rules of the game, i.e. by linguistic convention or stipulation – hence in a very Pickwickian sense of 'truth'. Mathematics would thus be a vast language – but one that does not talk about any entities.

Nominalism has attracted the formalists, who have worked primarily on the more abstract branches of mathematics, where axioms are the main, and sometimes the only, guide. However, not even Hilbert, the arch-formalist, was a full-blooded nominalist, for he admitted abstract objects and he acknowledged the need for (*a*) a notion of formal truth – inherent in his contributions to model theory, and (*b*) interpreted (*inhaltliche*) axiom systems side by side with uninterpreted ones (Hilbert and Bernays 1968 p. 2). Nowadays nominalists are to be found only in the Russian constructivist school (e.g. Markow 1975) and among philosophers.

The nominalist thesis about symbols as the objects of mathematics was refuted long ago by Frege (1895), who remarked that, precisely because symbols are chosen conventionally, they are inessential: what matters are the objects they stand for. (It is only *par abus de langage* that we write expressions such as 'Let $\mathbb{N}$ *be* the set of natural numbers' instead of 'Let '$\mathbb{N}$' *designate* the set of natural numbers'.) In sum, mathematical objects are not linguistic objects. (More on the symbol-concept distinction in Vol. 1, Ch. 1; on the numeral-number distinction in Kneale 1972.) As for the nominalist thesis about the purely "formal" or syntactic nature of mathematics, it was refuted by the emergence of the semantics of mathematics (Fraïssé 1982). According to the latter the meaning of a "formal system" (abstract or uninterpreted theory) is the set of its models (examples), i.e. the "structures" (systems) in which the axioms of the "formal system" (abstract theory) are satisfied – i.e. are formally (though not factually) true. However, the strongest objection to nominalism is perhaps that it demands an intolerable mutilation of mathematics. If it were taken seriously we would have to give up all the non-denumerable sets, such as $\mathbb{R}$ and its geometric counterpart, the line, for most of their members cannot be named: they are

concepts, not names. I.e., unlike the set of all possible names, $\mathbb{R}$ is non-denumerable.

Finally, the nominalist thesis that there are only individuals is sound with regard to material things, whereas the Platonic thesis of the reality of universals (such as "number" and "continuous") is inconsistent with factual science. But it is unreasonable to generalize that thesis to all objects – an unavoidable generalization given the tacit belief, common to both nominalism and Platonism, that there is but one type of existence. (Recall Sect. 2.1.) Modern mathematics rejects *both* the nominalist denial of universals and the Platonic thesis that universals (in particular properties) pre-exist individuals. In fact, the very definition of a class or species in terms of a predicate joins the two basic notions of universal and of individual: For any predicate F, Fa iff $a \in \{x|Fx\}$. In short, whereas factual science has vindicated the individualistic thesis with regard to the external world – though not the nominalistic prejudices against wholes and properties – mathematics has overcome the moninalism/realism dispute over conceptual objects ("abstract entities"). True, contemporary radical nominalists such as Nelson Goodman (1956) do not accept this solution and prefer to reject all versions of set theory. By so doing they remain alienated from modern mathematics.

As we intimated a moment ago, the nominalist thesis about symbols entails *conventionalism*. Now, conventionalism is utterly false with regard to factual science (Vol. 6, Ch. 12, Sect. 1.1), but it does have a grain of truth with reference to mathematics. In fact, it is true that (*a*) doing mathematics involves (though is not identical with) laying down and obeying certain definitions and rules, rather than conducting empirical tests or proceeding only inductively, and (*b*) mathematical truth is not established by empirical tests or by intuition but by playing the rules of deductive inference.

However, mathematical conventionalism has the following fatal flaws: (*a*) no mathematician ever adopts an axiom by arbitrary stipulation: axioms are adopted because of their fertility, and theorems are accepted when proved – and proved when interesting; (*b*) whereas linguistic conventions form a set devoid of a definite mathematical structure, mathematical statements tend to cluster into systems (theories); (*c*) no set of linguistic rules can help describe the world, whereas a mathematical theory, when enriched with suitable interpretation axioms, can; (*d*) unlike the rules of language, those of mathematics rest on laws (e.g. the factorization rule for numbers rests on the associative law); but not every law is a rule (e.g. an existence theorem does not tell us how to do anything: it just backs up our search for the object satisfying the theorem); (*e*) unlike chess and other games, "The best ma-

thematics is *serious* as well as beautiful [...] The 'seriousness' of a mathematical theorem lies [...] in the *significance* [importance] of the mathematical ideas which it connects. [...] Thus a serious mathematical theorem, a theorem which connects significant ideas, is likely to lead to important advances in mathematics itself and even in other sciences" (Hardy 1967 p. 89). In conclusion, nominalism is not an adequate philosophy of mathematics: it does not yield a faithful description of mathematics, and it has no heuristic power.

Next comes mathematical *intuitionism* (Brouwer 1975, Weyl 1949, Heyting 1956b, Bishop 1967, 1975, Dummett 1977). Unlike Platonists, intuitionists hold that the objects of mathematics are creations of the human mind. (Some intuitionists have even sketched an interesting theory of the creative subject who abides by the intuitionistic strictures: see Martino, 1982.) This ontological and psychological thesis should be adopted by anyone who is not an objective idealist, and it fits snugly into our naturalistic ontology. But admitting this thesis does not commit oneself to accepting the epistemological thesis that every genuine concept embodies an intuition of some sort. Indeed, whether or not a statement is intuitive is a highly subjective matter – partly because of the very ambiguity of the term 'intuition' (Bunge 1962a). For most people untutored in mathematics the following ideas are counterintuitive: that $1 = 0.999...$ (which results from adding 1/11 and 10/11, and taking the decimal expansions of these fractions); that there are infinitely many fractions between any two given fractions, however close; that the rationals are denumerable; that the numerosity (cardinality) of the rationals is as nothing by comparison with that of the reals; that there are non-associative objects, such as the Poisson brackets; that there are non-commutative operations, such as matrix multiplication; that there are curves, such as Peano's, that can fill a square. The thesis of the absolute character of intuitability is then empirically (psychologically) false. And the thesis that no unintuitive concept is valuable is dangerous because it hinders abstraction. It is not by chance that Grassmann's *Ausdehnungslehre* (1844), an early showpiece of abstract algebra and the origin of the vector calculus, was attacked by the Kantian philosophers of the time for being purely conceptual and unintuitive.

The methodological thesis of intuitionism, namely that the only legitimate way of introducing mathematical constructs is by explicit construction, is a horse of a different colour: in fact it is a respectable strategy of theory construction. In Sect. 5.1 we argued that it is an admissible foundational strategy provided it not be allowed to ban any alternative strategies. But the

constructivist thesis has no philosophical justification, for there is no *ontological* difference between "real mathematical existence" (proved by construction) and "pure mathematical existence" (generated by axiomatic definition or proved by *reductio ad absurdum*). In fact both are cases of conceptual or formal, not physical, existence. In this regard an individual number – be it small like 9 or enormous like $99^{99^{99}}$ –, the set of real numbers, and even the power set of the latter, are on the same footing: *all of them are fictions* (or ideal objects in the Platonic terminology). True, there are psychological, epistemological and methodological differences between those various types of mathematical object. But some of these differences are subjective and are being erased. For example, "enormous" numbers occur not only in some mathematical proofs but also in statistical mechanics and in cosmology. (E.g. the Poincaré recurrence time for 1 cc of gas is of the order of $10^{10^{19}}$ years.) In any event, getting down to manageable numbers takes us nearer familiar ground but not nearer reality: all mathematical objects are physically unreal. And numbers, whether large or small, are just as meaningless as sets, infinite or finite, because they are neither predicates nor propositions.

Intuitionists make much of the word 'meaning' in a rather vague sense that is neither the one used in model theory nor the one occurring in factual science. For instance Bishop (1975), following Brouwer, indicts "classical" mathematics for being largely "meaningless", and he insists quite rightly that one should not discuss matters of truth before having settled questions of meaning. And Stolzenberg (1970 p. 318) asks us to believe that "the principle of mathematical induction is correct *simply by virtue of its meaning*" – presumably because it refers to natural numbers. But neither Bishop nor Stolzenberg tells us what he means by 'meaning', aside from vague remarks about computability. (Besides, there seems to be a contradiction in Bishop's remark because, according to the operationist semantics adopted by intuitionism, nothing makes sense apart from testing procedures. On this view the meaning of a formula is the way it is put to the test: truth is the source of meaning rather than presupposing the latter. But this thesis is as false in mathematics as it is in physics: see Bunge 1973a, Ch. 1, and Vol. 2, Ch. 7, Sect. 5.1. The content of a statement cannot be identical with the way it is validated. For example, the Pythagorean theorem is not the same as the 200 or so different proofs of it.) In conclusion, although mathematical intuitionism contains some valuable epistemological insights, it is not an adequate comprehensive philosophy of mathematics. Therefore those mathematicians who call themselves 'intuitionists' do well to dissociate themselves

from the initial philosophical motivations of intuitionism. (See Bunge 1962a for further criticisms.)

Finally there is *mathematical empiricism*, which is shared by most dialectical materialists. (See e.g. Enriques 1913, 1940, Casanova 1947, Borel *apud* Fréchet Ed. 1967, Kálmar 1968, Davis 1974, Putnam 1975, Kuyk 1977, Lakatos 1978, Restivo 1983, Kitcher 1983, Harsanyi 1983.) Empiricism can be historical, philosophical, or both. The historical thesis of empiricism is that every construct originates, however remotely, in experience, so that there can be no such thing as pure mathematics. By leaving the adverb 'remotely' undefined, the thesis is irrefutable, for we can never be sure that there are no subtle threads linking even the most sophisticated abstract constructs to some sense experience. But taken literally the thesis is refuted by the great many cases where mathematical research was sparked off by problems of a purely conceptual kind: think of Diophantine equations, complex numbers, abstract algebra, non-Euclidean geometries, topology, or the theories of sets, real numbers, functional spaces, or categories. In all these cases the driving forces were curiosity and the wish to attain maximal generality or systemicity while preserving consistency. In short, historical empiricism is false.

The *philosophy* of mathematical empiricism boils to two theses. One is the semantical thesis that every mathematical object represent possible experiences or even "qualities of the world we live in" (Kuyk 1977 p. 137). The other is the methodological thesis that mathematical research proceeds much in the same way as empirical research, namely by trial and error, analogy, and induction, and that the ultimate criterion of mathematical truth is experience. A classical statement of the semantical thesis is found in Mill (1843 Bk. iii, Ch. xxiv, Sect. 5): "Each of the numbers two, three, four, & c., denotes physical phenomena, and connotes a physical property of those phenomena. Two, for instance, denotes all pairs of things, and twelve all dozens of things, connoting what makes them pairs or dozens; and that which makes them so is something physical". A more recent example is the attempt to reconstruct set theory in terms of concrete operations on perceptible things, such as collecting, ordering, and matching (Kitcher 1983 pp. 126 ff.)

To be sure there are a few examples like Mill's, which works for small positive integers, or like Kitcher's, which works for small collections of perceptible objects. But they are limited to the *early* stages of *some* branches of mathematics. There are many other cases where there is no reference at all to physical objects or to empirical procedures. Think of irrational

numbers (which cannot be the outcome of measurements), of continuous functions without derivatives (hence incapable of representing possible movements), of infinite (in particular divergent) series, or of abstract spaces. Besides, if every mathematical theory were just a summary and codification of experience, there should be but one arithmetic, one geometry, one set theory, one analysis, one logic, and so on. In fact there are many mathematics and a single world.

Even granting that *some* mathematical constructs have originated in sense experience or in manual practice, it does not follow that the proper way of investigating them is empirical. To hold this view is to incur the genetic fallacy. Geometry is not an empirical science just because it originated in land surveying, anymore than number theory employs empirical methods just because it started as counting fish or shells. If mathematics were an empirical science, theorem proving should be empirical not conceptual: for example, we should claim that the physical addition of things proves the laws for the addition of numbers – but then we should overlook counterexamples such as the juxtaposition of two liquid droplets, which may result in a single droplet, and the high energy collision of two elementary particles, which may result in a dozen particles. So, it is false that "the criterion of truth in mathematics just as much as in physics is success of our ideas in practice" (Putnam 1975 p. 61).

(Platonists, and perhaps moderate empiricists, would deny that we can "read mathematics off science", but they would argue that the interlocking of mathematics and factual science suggests that mathematics is about the world after all. Thus the eminent MacLane (1981 p. 471): "Mathematics consists in the discovery of successive stages in the formal structures underlying the world and human activities in that world, with emphasis on those structures of broad applicability and those reflecting deeper aspects of the world". In our view, the facts that mathematics is the heart of modern theoretical factual science, and that the latter has often stimulated the former, prove not that mathematics is just as *empirical* – in origin, essence, and method – as factual science, but that the latter is *rational* as well as empirical. In any event, either thesis has got to be justified by a semantical analysis of the reference of mathematical concepts such as "$\in$" and "$\mathrm{d}/\mathrm{d}x$". We have done this job in Vols. 1 and 2, which show that $\in$ refers to sets in general and $\mathrm{d}/\mathrm{d}x$ to functions in general. On the other hand Platonists and empiricists still owe us viable semantic theories capable of disclosing the referents of any predicates or propositions, and of substantiating their claim that mathematics studies the structure of the world. And neither can

possibly account for the recent finding that there are at least two different systems of real numbers, so that reality contains neither of them.)

The doctrine that mathematics mirrors human experience, or even the world, is open to the following additional objections: (*a*) no mathematical proof represents anything in reality, if only because the deducibility relation has no material counterpart; (*b*) by itself no mathematical formula represents anything in reality: so much so, that one and the same formula, such as a differential equation, can be assigned a number of alternative factual interpretations; (*c*) take a process such as a macrochemical reaction, on the one hand, and the equation in chemical kinetics representing that reaction, on the other: the equation contains a parameter (the rate constant) that must be determined empirically, and every term in the equation is assigned a factual reference by an extramathematical assumption ("correspondence rule"); (*d*) whereas physical (or chemical, biological, or social) links are factual (think of pushing or burning), mathematical relations are not: they have no causal efficacy.

Yet in recent times it has become somewhat fashionable to claim that mathematics, even logic, can be "read off science". In particular it has been held that quantum mechanics has refuted standard logic in the microphysical domain. (Recall Sect. 3.2.) Most authors rest this claim on an examination of a couple of experiments such as the double slit diffraction experiment. But, far from carrying out the calculation within the framework of an axiomatic formulation of quantum mechanics – in order to be able to ascertain what the underlying logic is – they take the theory as usually formulated and work from then on. Had they proceeded rigorously, i.e. by first axiomatizing the theory, they would have realized that it makes exclusive use of theories in classical mathematics, which in turn presuppose classical logic. (See e.g. Bunge 1967c.) Hence any seeming violation of ordinary logic in microphysics must be a mistake in the application or the interpretation of the mathematical formalism of quantum theory. (Recall Sect. 3.2.) In conclusion empiricism is a false philosophy of mathematics – except for the platitude that mathematical work is a type of human experience. (Further criticisms in Goodstein 1969, Goodman 1979, Bell and Hallett 1982.)

This concludes our critical survey of the four classical philosophies of mathematics, which will be found summarized in Table 1.1.

Every one of the classical philosophies of mathematics has some good points, but none of them covers adequately all aspects of mathematical research: posing and reformulating problems, using theories or guessing

TABLE 1.1. Typical tenets of some philosophies of mathematics.

Philosophy	Math. objects	Mode of introduction	Meaning	Truth	Math. knowledge	Math. activity
Platonism	Self-existing ideal and eternal	Discovery	Non-contradiction	Formal	A priori and conceptual	Deductive
Nominalism	Symbols	Convention	Nil	Convention	Nil	Formal manipulation of symbols
Intuitionism	Mental constructions	Invention	Reducibility to positive integers	Reducibility to numerical computation	A priori and intuitive	Intuitive and rational
Empiricism	Mental	Discovery	Reference to experience	Empirical	Empirical	Trial and error, rational and empirical
Conceptualist and fictionist materialism	Fictions (classes of brain processes)	Invention and discovery	Conceptual reference and contextual sense	Formal	A priori and conceptual	Abstraction, generalization, formal manipulation, trial and error, analogy, induction and deduction

hypotheses to solve them, proving theorems, inventing axioms, definitions and algorithms, computing, comparing constructs, making mathematical considerations, etc. – all the while using intuition, analogy, induction, and deduction. Our own philosophy of mathematics, sketched in the preceding pages, was intended to retain those valid points and to cover the main aspects of mathematical research as well as of its finished products. In fact

(i) it accounts for the purely conceptual nature of (finished) mathematical objects and methods, while making room for the empirical or intuitive origin of some of them;

(ii) it accounts for the universality and impersonality of mathematical constructs while stressing that they are brain-children likely to be created only in fairly advanced societies;

(iii) it accounts for the differences between formal and factual propositions, as well as between mathematical proof and empirical validation;

(iv) it accounts for the difference between an abstract theory and its models as well as between logical models and the models in science and technology;

(v) it accounts for invention (of new constructs), discovery (of logical relations), and routine work (in applying rules or algorithms);

(vi) it accounts for the applications of mathematics without rendering it the handmaiden of science and technology: service but not servitude;

(vii) it leaves maximal freedom compatible with consistency and systemicity (relatedness to other ideas);

(viii) it respects the logical stratification of mathematics (elementary logic, category theory, set theory, number theory, abstract algebra, topology, analysis, etc.);

(ix) it requires neither mythical objects, such as self-existing Platonic ideas, nor non-rational faculties, such as intuition (except as a heuristic aid);

(x) unlike all other philosophies of mathematics, it is part and parcel of a comprehensive philosophical system consistent with science and technology.

6. Concluding remarks

Like any other human activity, mathematical research and its product can be looked at in a number of alternative ways. One is the logical viewpoint, which regards mathematics as a finished product and, more particularly, as a system of theories. Another is to focus on mathematical activity – its motivations, modes of reasoning, etc. – as a particular kind of mental (or

brain) activity; this is the psychological viewpoint. A third is to look at mathematical research as a type of social activity, and at its product as a special kind of cultural artifact: this is the sociological viewpoint. A fourth is to study mathematics as a historical process of discovery, invention, and diffusion throughout a given society: this is the historical viewpoint. A fifth is to study mathematics as a tool for factual science, technology, and the humanities: this is the instrumental viewpoint. A sixth is to look upon mathematical research as a particular mode of cognition, and its product as a special kind of knowledge: this is the epistemological approach.

These different ways of looking at mathematics are mutually compatible, nay complementary. Therefore it would be wrong to espouse one of them and exclude all the others, for mathematics is simultaneously everything those alternatives say it is. However, the epistemological viewpoint is methodologically prior to the others because it is the one that tells mathematics from other disciplines. (Imagine trying to trace the history or the social ties of a mathematical idea without first making sure that it is indeed *mathematical* rather than scientific, technological, or philosophical.) Still, the philosopher cannot do a good job of characterizing mathematics unless he takes the other points of view into account. A couple of examples will show the need for such extraphilosophical information.

Example 1 Consider the formulas "2 + 3 = 5" and "5 = 3 + 2". They are mathematically identical – except of course to the nominalist, since they are typographically different. But they are psychologically (or pragmatically) different: the first may be interpreted as the instruction to "take" (think of or handle) first the number (or the numeral) 2, then add it 3; on the other hand the second formula may be construed as an instruction to decompose 5 into 3 and 2 (in this order, not the reverse). The possibility of such an extramathematical interpretation helps explain the survival of the empiricist (in particular pragmatist or operationist) philosophy of mathematics. *Example 2* At first sight mathematics is the lonely profession in the sense that it is rarely the work of teams, it is unpopular, and it is hard to popularize. However, mathematics is just as dependent upon tradition and communication as any other research field. It is not only that the information channels must be kept open in order for mathematics to flourish, but that some of its key ideas seem to have been generated in social intercourse. Thus the idea of a proof may have been necessitated by the need to persuade somebody (Hammond 1978 p. 30). And the idea of consistency may have originated in the need to attain consensus in a political democracy. (Politics, not the economy, explain why logic was born in Greece rather than in any

of the despotic slave states.) This is no mere matter of historical erudition: philosophers should know more about the psychological and social mechanisms of validation of truth claims in order to understand why the rules of inference and the standards of rigor are anything but absolute.

Given the nature of this book we have adopted an epistemological approach to mathematics. However, from the beginning we have stressed the anti-Platonist thesis that mathematics does not exist except in the brains of some people living in societies that encourage or at least tolerate free speculation and the free discussion of ideas. How can this materialist view jibe with the fictionist component of our philosophy of mathematics, according to which when creating or utilizing a mathematical construct we *feign* that it leads an impersonal and suprasocial existence? There is no contradiction here, for we hold that it is we, living beings immersed in a concrete society with a rich tradition, who construct such fictions. (For a critical examination of our brand of fictionism see Torretti 1982.)

Still, is not such fictionism regarding mathematics inconsistent with the scientific realism espoused in Vols. 5 and 6? No, because our fictionism, unlike that of Nietzsche or Vaihinger, is limited to fictions such as mathematical systems and mythologies: it does not carry over to factual science and technology. In other words, our epistemology is realistic concerning the study of the real world, and fictionistic concerning fictions. It is thus dualistic in that it preserves Leibniz's distinction between *propositions de raison* and *propositions de fait* (Sect. 1.1). Hence it is more complex than its rivals, most of which are monistic in assuming that there is a single kind of existence, hence of proposition. Yet, epistemological dualism is not self-contradictory. It is consistent for it does not predicate fictitiousness of factual scientific constructs (e.g. the equations of chemistry) or material reality of mathematical fictions. (That there is often a correspondence between them, as between atoms and the equations representing them, is another story.) Moreover such epistemological dualism does not carry over to our ontology: this one is thoroughly monistic because it does not postulate that constructs are part of the furniture of the world. What are found among the furnishings of the real world are brains capable of creating constructs.

There is much more to the philosophy of mathematics, but we must keep going. Henceforth we shall only encounter mathematical objects as components of our ideas about reality.

PHYSICAL SCIENCE FROM PHYSICS TO EARTH SCIENCE

We start now our study of some topical problems in the philosophy of factual or empirical science. As we saw in Ch. 1, factual science is semantically and methodologically quite different from mathematics. Unlike mathematics, factual science seeks truths of fact – i.e. propositions representing reality – and it employs not only mathematical reasoning but also empirical procedures. However, factual science and mathematics share a number of traits, and this is why both are rightfully called 'sciences': both employ the general scientific method, both attempt to maximize exactness and systemicity, both justify their assertions, and both change relentlessly as a result of research rather than in obedience to dogma or social pressure. (Recall the general definition of a scientific research field in Ch. 1, Sect. 1)

We begin with the philosophy of physics for the following rather obvious reasons. Firstly, physics is the most basic and universal of all factual sciences, because (*a*) every real thing is either a physical entity or is composed of physical entities, and (*b*) the laws of physics are not place or time bound. Secondly, physics (including astronomy) was the first discipline to attain a scientific status and, as a consequence, it showed the way of science to all the other research fields. Thirdly, the philosophy of physics (including astronomy) is the oldest, most firmly established and most flourishing of all the philosophies of science. (Ptolemy was one of its founding fathers, and Galilei its first modern practitioner.) For this reason the philosophy of physics showed the way to the other regional philosophies of science. This has been both a blessing – because of the high standards it required – and a misfortune – because we have tended to overlook the specific differences between the physical sciences and the others. (Biologists often complain that philosophers of science treat their science as if it were a mere extension or application of physics. See Ch. 3, Sect. 1.1.)

Although the philosophy of physics is the most advanced branch of the philosophy of science, it is still underdeveloped in a number of respects. For one thing much of it is based on secondary literature. Secondly, philosophers of physics are often overspecialized – e.g. in the philosophy of space

and time, or of quantum mechanics. Consequently they tend to take problems out of their general scientific setting, which prevents them from controlling the proposed solutions by checking their compatibility with the bulk of physics. Thirdly, philosophers of physics seldom attack problems in the light of an explicit semantics, epistemology, methodology, and ontology. Consequently they often come to conclusions that are at variance with the marrow of physics – witness the views that the world is a system of mathematical equations, that there are no laws but only conventions, that there is a general and a priori theory of measurement, that change is illusory, or that physics does not study autonomously existing things. Fourthly, although many workers in the philosophy of physics master some of the mathematics of physics, they seldom make use of the mathematics of philosophy. As a consequence their assertions about exact formulas are often inexact for including unanalyzed key philosophical or methodological concepts, such as those of matter, event, causation, physical quantity, factual meaning, factual truth, and measurement.

There is, then, much to be done in the philosophy of physics. In this chapter we shall grapple with a handful of open problems, and shall do so by availing ourselves of some of the tools wrought in previous volumes of this Treatise. On occasion we shall also make use of results, in physics and its philosophy, obtained by the author or his coworkers. The first Section will be a *pot pourri* devoted to elucidating half a dozen key concepts of physical science. These concepts are so familiar that they are seldom analyzed – a sure sign of their scientific importance and philosophical complexity.

1. PRELIMINARIES

1.1. *Physical Quantity, Convention, Measurement*

Physical science studies physical things such as bodies and fields. (This platitude is of course denied by subjectivists, operationists and conventionalists.) Physical things can be simple – i.e. without separable components – though possibly extended; electrons and photons seem to be both simple and extended. Or they can be complex, i.e. systems proper such as atoms and clouds. All things other than physical things are systems, and they exist only in special environments. For example, chemical systems are confined within rather narrow temperature and pressure bounds, biosystems

exist only in a narrow range of habitats, and sociosystems under even more special circumstances. (Recall Vol. 4.)

The properties of a physical thing are appropriately called 'physical properties'. If a thing has properties other than physical properties then it is not a physical thing and therefore it lies beyond the reach of physical science. (The latter may on the other hand study the physical components of a supraphysical thing.) This truism is at variance with the subjectivistic interpretation of certain physical theories, particularly quantum theory. According to that interpretation there are no physical things independent of human thought and action. Consequently physics would be indistinguishable from psychology or even epistemology: it would study human knowledge rather than nature. This view, which stems from Berkeley and Mach, will be examined in Sect. 4.2. Suffice it to state here that it is at variance with the practice of physical research. Indeed the theoretical physicist attempts to model physical things in terms of purely physical properties, and the experimental physicist racks his brains to keep himself at arm's length from the things he studies in order to minimize the disturbances he may cause.

All physical things, however simple, have numerous properties; the one property they all share is presumably energy. All the properties of a physical thing-in-its-environment are lawfully interrelated. The central goal of theoretical physics is to conceptualize such properties and the constant relations among them, i.e. the laws of physical objects. And the central aim of experimental physics is to measure physical properties, explore their inter-relations, and test plausible conjectures about the latter.

Every property is conceptualizable (representable by) at least one physical "quantity", whether constant or variable. In turn every such "quantity" is either a function (e.g. the temperature function) or an element of an algebra – e.g. the spin operator. (We distinguish a function from its values, and an element of an algebra from its spectrum.) For example, the value of the temperature of a macrophysical system in a given state (and reckoned on the absolute temperature scale) is some non-negative real number. Ordinarily, the values of physical properties are n-tuples of real numbers. But the converse is false. Thus spacetime is currently represented by a differentiable manifold, every point of which is identifiable (relative to a given coordinate system) by a quadruple of real numbers. But these numbers are not the values of any functions. This is not surprising, because spacetime is not a physical property of any particular physical objects: it is the public property of all. (On the other hand the spatial position and age of a physical object can be construed as the values of certain functions.)

It will pay to be more precise and examine a couple of examples. Our first example will be one of the simplest physical "quantities", namely electric charge. This is an intrinsic property of some bodies, i.e. one that does not depend upon any other physical object; in particular it is invariant with respect to changes in reference frames. Electric charge can be conceptualized as a function from the collection of all possible bodies to real numbers. (Note that (*a*) we employed the word 'collection', not 'set', for we are dealing with a variable collection not a fixed set; (*b*) we refer to all possible bodies, not just those that happen to exist right now relative to our time frame; (*c*) our collection of bodies includes not only those that happen to be electrically charged (the current extension of the concept of charge) but all possible bodies, for even the ones that are now electrically neutral may acquire an electric charge.) However, we can reckon or measure values of electric charges in alternative charge units, e.g. the e.s.u. and the coulomb. Therefore we must write "$Q(b, u) = r$" for "The value of the charge of body b, in unit u, equals the real number r". The general concept of electric charge is then

$$Q\colon B \times U_c \to \mathbb{R},$$

where B is the collection of possible bodies, U_c that of conceivable charge units, and $\mathbb{R}$ the system of real numbers.

(Note that the electric charge on a body is a fact, even though the unit in which it is calculated or measured is conventional. Also, it is a fact that positrons and electrons have the same charges with opposite signs, though calling the former 'positive' and the latter 'negative' is mere convention. All this is trivial. What is not trivial is the matter of the transformation properties of the electric charge, in particular its behavior under orientation reversal, i.e. $x \to -x$, and time reversal, i.e. $t \to -t$. The usual assumption is that electric charge is invariant under these operations, i.e. that it is an absolute scalar. However, it is occasionally assumed that Q is reversed with time reversal, or that Q is reversed with space inversion. The problem is still open and it cannot be solved by studying it in a single branch of physics, as the charge concept occurs almost everywhere. See Post 1977.)

A slightly more complex example of a physical quantity is that of position. We can write "$P(b, f, t, u) = \langle x, y, z \rangle$" for "The position of point-like object b, relative to reference frame f, at time t, and in unit u, is the ordered triple of real numbers $\langle x, y, z \rangle$". In general, the (classical) concept of position coordinate is a function of the form

$$P\colon B \times F \times T \times U_d \to \mathbb{R}^3,$$

where now B is the collection of point-like objects – or of points of an object, such as a wave-front –, F the collection of reference frames, T that of instants, U_d that of distance units, and $\mathbb{R}^3$ is the collection of ordered triples of real numbers. In the quantum theories there are several position operators. (See Kálnay, 1971.)

The following points of methodological or philosophical interest are worth mentioning. First, physicists distinguish physical properties from their mathematical representatives. This distinction is usually tacit, but it is explicit in the quantum theories, where one talks about the operator $\hat{A}$ representing the dynamical variable (or "observable") A. Obviously, such a distinction would make no sense at all if physics were wedded to an idealistic epistemology. (More on the property-attribute distinction in Vol. 3, Ch. 2, Sect. 1.1.)

Second, it is common for a given physical property to be represented by different functions or operators – or by none at all – in different theories. For example, relativistic mass, unlike classical mass, is frame-dependent; and in relativistic quantum mechanics the relative mass is represented by an operator. In other words, the "quantity"-property relation is many-one. (Equivalently: let $\mathbb{P}$ be a set of physical properties, and V the totality of "quantities" representing them. Then the representation function is of the form ρ: $2^V \to \mathbb{P}$, where 2^V is the power set of V.) This fact shows the need for distinguishing between objective properties and the attributes or predicates representing them. The idealist and the naive realist do not need this distinction; by the same token they cannot account for its occurrence in physics.

Third, there is no mathematical difference between a physical "quantity" and a function (or operator or an element of an algebra). The difference is made in an extra assumption of a semantic nature. Thus in the case of the electric charge function Q treated above we assert that $Q(b, u)$ *represents* the electric charge, in unit u, of body b. In our terminology this is a *semantic assumption*, known as a "correspondence rule" in the literature. The difference in terminology corresponds to a philosophical difference: (*a*) whereas according to positivism a "correspondence rule" correlates symbols with empirical operations, in our view a semantic assumption correlates concepts with objective properties that may or may not be measured with different techniques; (*b*) whereas on positivistic metascience these correlations are rules or instructions for interpreting symbols (so as to endow them with an empirical content), on our philosophy they are hypotheses and, as such, they are empirically testable (though not in isolation). For example, the supreme

"correspondence" rule of the Copenhagen interpretation of quantum mechanics is that every formula of this theory is about some experimental situation, none about anything in itself. (More in Sect. 4.2.) This empiricist principle is refuted every time a physicist makes a calculation referring to an isolated thing, or a measurement on an object lying beyond the reach of his laboratory.

Fourth, the mathematical representation of a physical property may contain conventional elements, such as units (sec, volt, degree, etc.). Only dimensionless magnitudes, such as relative increments and the probabilities of states, fail to contain units. (For a mathematical theory of dimensions and units see Bunge, 1971.) Units are not the only conventional components of physical theories. Another conspicuous component that can be chosen conventionally is the kind of coordinate system – e.g. rectangular, spherical, or cylindrical. (A coordinate system is not to be mistaken for a reference frame: the former is a concept, the latter a physical thing, such as a semi-rigid body with a clock attached to it. Any given reference frame can be represented by a number of alternative coordinate systems.) It might be thought that the occurrence of such conventional components refutes realism and confirms conventionalism. This is like saying that, since all painting includes conventional components, portraits have no real subjects; or that, since the GNP of a nation can be reckoned in dollars, yen, or rubles, there is no such thing as GNP. Unavoidably, every conceptual representation of a real thing is made up of concepts, and some concepts may represent things more or less faithfully, but they are not identical with the things represented.

Our analysis of a physical magnitude serves to identify its conventional component(s). And that analysis, coupled to our theory of reference (Vol. 1, Ch. 2), helps us exhibit the real referents of any physical magnitude. Thus, the real (or factual) reference class of the concept Q of electric charge, studied a while ago, is the collection B of possible bodies, which is properly included in the total reference class $B \cup U_c$ of Q. Similarly the real referents of the concept P of position coordinate are physical objects and, in particular, reference frames. (The total reference class of P is $B \cup F \cup T \cup U_d$. Since time does not exist by itself, and the distance units are conventional, we discard T and U_d when forming the factual reference class of P.)

Fifth, all things have a number of properties, presumably a finite number of them. (However, the values of these properties are different in different states – by the very definition of "state" – and relative to alternative reference frames. For example, the mass of a body has as many possible values as the values of its velocity relative to all the possible inequivalent

reference frames.) But not all properties are equally important or of equal interest for a given purpose. Hence the problem of choice. This problem is solved differently by people with different interests. Thus the technician and the man in the street prefer things with a small number of easily measurable or controllable properties – such as the three variable system in Figure 2.1. A state of this system is a consistent triple $\langle a, b, c \rangle$ of angular positions of the pointers showing the values of the properties A, B, and C. On the other hand the experienced scientist knows that things are always more complicated than they look at first sight, and that the most important properties are often the most difficult to measure or control. So, he may investigate any "well known" thing and, with talent, industry and luck, he is likely to discover further properties of it. If he is a theorist he will not care much for ease of measurement or control – which are on the other hand prime considerations of the technologist – because he knows that they depend upon the state of the art as well as upon the thing itself. He knows that certain properties, such as spin, stress, and space curvature, are hard to measure as well as important.

An important or basic property of a thing is one upon which other – derivative – properties depend: recall Vol. 3, Ch. 2. Important properties, or rather the magnitudes representing them, occur or should occur in law statements such as equations of motion and field equations. And a property is the more important, basic, or fundamental, the more fundamental the law statement(s), or their deductive consequences, in which its representative occur(s). In turn, the degree of fundamentalness of a law statement in a theory is given by its rank in the theory. Thus a variational principle is more basic than the equations of motion, or the field equations, it entails; and the latter are more basic than any of their solutions. As with properties so with law statements: the most general among them are the more important or

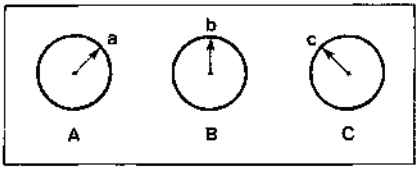


Fig. 2.1. Control panel of a three variable thing. The thing happens to be in state $\langle a, b, c \rangle$. The values a, b, and c form a consistent triple in that they are related by the law(s) of the thing.

basic. For example, the general equations of motion in continuum mechanics are more important than the constitutive equations, which inter-relate some of the properties of bodies of special kinds. However, no realistic special problems can be solved without using some such special laws, and the technologist can often get by with the sole help of the latter. Thus the petroleum engineer may set more store on the constitutive equations that characterize oil than on the general equations of hydrodynamics; likewise the electrical engineer may find more use for the special laws of circuits, such as Ohm's and Kirchhoff's, than for Maxwell's field equations. This is just one example of the difference between what is scientifically important and what technologically important: between knowing for its own sake, and knowing for the sake of doing. See Vol. 6, Ch. 14, Sect. 2.2.)

Sixth, exhibiting the general mathematical form of a magnitude, e.g. as a real valued function of two variables (i.e. $F: A \times B \to \mathbb{R}$), tells us very little about the magnitude. We also need to know the so-called rule of assignment of values to every point in the domain of the function (i.e. what exactly is the value of the F above at the point $\langle a, b \rangle$ in $A \times B$). In physics these "rules" are the law statements linking the given magnitude with other magnitudes. For example, Ohm's law, a constitutive equation, states that the possible values of the electric current in a d.c. metallic circuit are proportional to the impressed electromotive force. In general, the (nomologically) possible values of a magnitude are determined by the entire set of hypotheses (e.g. equations) that define (implicitly or explicitly) the magnitude.

Seventh, the law statements determine only possible values of magnitudes. The actual values depend on the actual special circumstances and they must be found out by measurement. As we saw in Vol. 6, Ch. 11, Sect. 3.1, measurement can be direct or indirect, i.e. with the help of well confirmed hypotheses, among which indicator hypotheses play a crucial role. For example, the length and period of oscillation of a simple pendulum can be measured directly (though not very accurately). And these values can be plugged into a well-known formula to yield the local acceleration of gravity.

Let us have a closer look at physical measurement, for it poses some philosophical problems that have hardly been explored. Whereas theoretical physicists deal most of the time with physical objects in themselves, such as protons and galaxies, regardless of the ways they can be observed or experimented on, experimental physicists handle physical objects "for us" (the knowing subjects). Indeed, they design, assemble (or construct), and operate measurement systems in which the physical objects of interest are

included as components. We recall from Vol. 4, Ch. 1, that a system may be characterized by its composition, environment, and structure. In particular, a *measurement system* may be described as follows:

(i) *composition*: a *mensurandum* or object of measurement (e.g. an atom or a light beam), an *apparatus* (e.g. a seismograph), and an *experimenter* (or his automated proxy);

(ii) *environment*: an artificial milieu the main properties of which are carefully monitored and controlled, and that is maximally shielded from perturbations coming from the experimenter as well as from the part of the world that is not being studied;

(iii) *structure*: the collection of links among the system components, and among these and environmental items. In a well-designed measurement system, the salient interaction is that between mensurandum and apparatus: the influences of the experimenter and of the environment are negligible or can be corrected for. More on this anon.

In turn, the apparatus can be analyzed into the *measuring* instrument(s) – e.g. a pendulum – and the *recording* device(s) – e.g. a pen attached to a pendulum and writing on a strip of moving graph paper. The measuring instrument must be sensitive enough to detect small changes in the state of the mensurandum. And, when triggered by the latter, the instrument should be capable of causing an *irreversible amplification* in the recording device – e.g. an ink trace, a spot on a photographic plate, or an electric discharge.

It goes without saying that both the detection and the recording processes are assumed to be purely physical. In particular, psychokinetic effects are excluded by hypothesis for, if admitted, the measurement would be rigged and therefore worthless. In general, the role of the experimenter is limited to the design and operation (personally or by proxy) of the measurement system. Once the measurement system has been set up, the experimenter is supposed to keep at arm's length from it; moreover he must be able to be replaced by any other competent individual. Otherwise the measurement results would not be replicable and consequently they would be suspect.

Since the experimenter is not a permanent feature of a measurement system, any more than the mensurandum, and since the mensurandum-apparatus interaction is assumed to be strictly physical, it is mistaken to claim (as does Wigner 1963) that consciousness plays an essential role in the measurement process. If consciousness did play an important role in measurement, the latter would not be measurement. As Wheeler (1978) put it, "not consciousness but the distinction between the probe and the probed" is central to scientific observation. This distinction is denied by the standard

quantum mechanical theory of measurement (von Neumann 1932, London and Bauer 1939), according to which the border between the knowing subject and the physical object is arbitrary. More on this point in Sect. 6.1.

Although every competent experimentalist takes pains to avoid interfering personally with the measurement process, he may deliberately design or use measuring or recording devices that produce strong perturbations on his mensurandum, to the point of altering it qualitatively. For example, to localize a photon he may resort to a photographic plate that will absorb it. And when using a Stern-Gerlach apparatus to measure electron spins, he actually causes the polarization of the incoming electrons, which are normally unpolarized (i.e. in a superposition of spin eigenstates). In fact the magnetic field of the Stern-Gerlach apparatus (but not the observer by himself) projects the spins onto the field axis, some in the same direction, others in the opposite one. (More in Sect. 6.1.)

Needless to say, any disturbances caused by the apparatus on the mensurandum are strictly physical, hence describable with the help of physical theories: they are not effects of mind on matter. Nor do they prove that scientists study only what they make, let alone that they conjure up all the events in the world. Whatever changes are caused by a measurement apparatus on a mensurandum are not only strictly physical but also strictly lawful: there is nothing arbitrary about them. (The projection of the state function is an apparent exception. We shall see in Sect. 6.1 that it is a consequence of lawful interactions.)

In other words, there are limits to what the experimentalist can do. For example, he cannot transform an electron into a neutron, or bring a photon to rest without destroying it, or change an invariant property (such as electric charge or entropy) by merely adopting a different reference frame. The Copenhagen interpretation of quantum mechanics denies both the objectivity and the lawfulness (hence the inherent limitations) of microphysical events: it asserts dogmatically that all of the quantum events are freely "conjured up" by the experimentalist (e.g. Bohr 1934, Rosenfeld 1953).

The design, performance and interpretation of precision physical measurements calls for *measurement theories*. There are two families of such theories as regards generality and testability. The first, invented by mathematical physicists and widely discussed by philosophers, is composed of extremely general theories involving the principles of quantum mechanics. They have no classical correlates and they attempt to account for the measurement of arbitrary physical magnitudes with the help of nondescript

apparata and arbitrary techniques. Being so unrealistic, these theories make no specific predictions and consequently they remain untestable – which is a very convenient feature because they can be multiplied *ad libitum* and discussed to no end.

The second family of measurement theories, produced by theoretical physicists or experimentalists turned theorists, is composed of specific theories. They are usually in the *Review of Scientific Instruments* rather than in textbooks. (Since they are all based on general theories, such as mechanics, wave optics, or thermodynamics, they are bound models in the nomenclature of Vol. 5, Ch. 9, Sect. 1.2.) These theories account for the way particular properties (e.g. acceleration, viscosity, electric field intensity) are measured with the help of particular pieces of apparatus. Such specific theories are classical, quantum-mechanical, or semiclassical. *Examples*: the theory of Millikan's oil drop method for measuring the charge/mass ratio; the theory of the way the Geiger counter works; the theory of the mechanism whereby a photographic plate records the passage of a charged particle. Unlike the general measurement theories, the specific ones are testable and, moreover, they are used to design and operate measurement systems, as well as to improve them. But, since we have started to talk about theories, we are already trespassing on the next section.

1.2. *Theory, Metatheory, Protophysics*

Contemporary physics is essentially composed of theoretical physics and its assistant, mathematical physics, together with experimental physics and its own assistant, instrumentation. Physicists usually specialize in either of these fields. (Enrico Fermi was probably the last great physicist who felt at home in all four subfields.) The specialist in instrumentation and the mathematical physicist ignore each other, but the theoretical physicist is supposed to hear about experimental results once in a while; and the experimental physicist must have an intuitive grasp of a number of theories, for he uses some of their results to design and interpret his experiments. Because philosophers have a theoretical cast of mind, they have generally overlooked the rich philosophical problematics of experimental physics and have concentrated instead on a few theories, mainly classical particle mechanics, the two relativities, and quantum mechanics. (Continuum mechanics, statistical mechanics, classical electromagnetic theory, quantum electrodynamics, theoretical solid state physics, and many other theories are still waiting to be explored philosophically.)

The theories and theoretical principles of physics are not only numerous

but they are also of different kinds. (All theories contain principles, or postulates, but not all principles are components of theories. More on this below.) In fact we may distinguish the following levels of physical theorizing:

(i) *Protophysical* principles and theories (e.g. physical geometries)
(ii) *General theories*
  (*a*) fundamental (e.g. classical mechanics)
  (*b*) phenomenological (e.g. classical thermodynamics)
(iii) *Special theories*
  (*a*) about natural things
  (*b*) about artifacts (e.g. measurement systems)
(iv) *Metatheoretical principles*

We call *protophysics* a motley collection of more or less tacit principles, as well as of explicit theories, that underlie theoretical physics (Bunge 1967c). Because protophysics belongs in the foundations of physics and is largely tacit, it behooves the philosopher to ferret it out, analyze it, and systematize it. We have done some of this job in Vol. 3, which was devoted to things in general, i.e. regarded as physical objects. But there we did not touch on any of the rather special protophysical principles presupposed by the very methodics of physics. (See Bunge 1967c for some of them.) Let us now have a look at one such principle, which has played a central role in the recent philosophy of space and time: the isometry hypothesis. But before we do so we must recall what a measurement standard is.

All physical measurements involve the use of *standards*, e.g. of time or of mass, which embody units of magnitudes. Standards are either things (such as the classical mass standard) or processes (such as a time standard). They are natural, such as a spectral line, or artificial, such as a battery. All standards are easily observable or replicable, and they are assumed to be constant under specified circumstances. Thus an atomic length (or time) standard is assumed not to change in the course of time provided the atoms that compose it are kept at constant temperature and pressure. Being factual items, standards are describable but not definable. On the other hand units (e.g. of time) are conventional; moreover, the derived units are definable in terms of the basic ones with the help of law statements. (For example the newton, a force unit, is definable via Newton's second law of motion in terms of certain units of mass, length, and time.)

According to Mach, Poincaré and their followers – in particular Reichenbach and Grünbaum – standards are thoroughly conventional. In particular, the length constancy (isometry) of a length standard would be convention not fact. This opinion, crucial to the conventionalist philosophy of space

and time, was first formulated by Poincaré and was elaborated in recent years at great length by Grünbaum (1973). This view is mistaken: the isometry hypothesis is not an arbitrary convention but a well founded conjecture. Indeed, isometry is based on well confirmed physical theories that account for it. Thus the isometry of atomic time standards (the most accurate of all standards) is explained by the atomic theory. Indeed, the latter shows that the frequencies of the light waves emitted by the atoms constituting the standard depend only upon the variables that happen to be kept constant (pressure and temperature). In other words, statements such as "The atomic time standard has a constant period (at constant pressure and temperature)" are neither conventions nor raw experimental data, but well founded and well confirmed hypotheses representing certain isometric facts. The conventionalist mistake might have been avoided if standards had been carefully described and distinguished from units. Correcting that mistake destroys one of the pillars of the conventionalist philosophy of space and time, to which we shall return in Sect. 3.

The next item in our agenda is general physical theory. Every such theory poses a number of philosophical problems, some of which we have studied earlier (e.g. Bunge 1967c, 1973a, 1983b). Here we shall touch only on one of them: the difference between fundamental (or mechanismic) and phenomenological (or black box) theories. (See also Vol. 5, Ch. 9, Sect. 1.2.) *phenomenological theories*, such as the elementary theory of electrical circuits and classical thermodynamics, describe large masses of facts without explaining them, i.e. without accounting for the mechanism of their occurrence: to indulge in slang, they tell it like it is, not why it is so. Every phenomenological theory contains magnitudes that are readily accessible to measurement. (However, many of them also contain magnitudes that can be calculated but not measured in a direct fashion. For example, thermodynamics contains, among other unobservables, entropy and specific heat at constant volume.) Consequently the phenomenological theories can be confronted rather directly with laboratory data. (For this reason they are the darlings of radical empiricists, such as the classical positivists.) But the price paid for empirical testability is high. For one thing phenomenological theories are only skin deep. For another they contain parameters that do not represent any physical properties and that have to be determined experimentally.

Fundamental theories, on the other hand, contain no such parameters. All the variables and constants occurring in them represent, more or less symbolically, physical properties. For example, Dirac's relativistic quantum

mechanics contains only four constants, all of them universal and with a reasonably clear physical interpretation: the values of the electron mass and charge, the velocity of light in vacuum, and Planck's constant. Yet paradoxically, although fundamental theories represent facts more directly than phenomenological theories, they are far more remote from raw laboratory data. This is due to the fact that they are not about observable facts. The function of fundamental theories is not to fit empirical data but to explain phenomenological laws and theories. For example, Newtonian mechanics explains Kepler's laws, and quantum mechanics explains phenomenological solid state theory. Because of their depth and their remoteness from raw data, fundamental theories have often been opposed by empiricists.

Another important point usually glossed over by philosophers is the difference between general and special theories. A *special theory* is obtained from a general one (when available) either by dropping or by specializing certain hypotheses. (In the first case the special theory is a subtheory of the more comprehensive one. Thus statics is a subtheory of dynamics, and electromagnetism *in vacuo* is a subtheory of electrodynamics.) For example, particle mechanics, rigid body mechanics, hydrodynamics, and celestial mechanics are so many specializations of continuum mechanics. (Every such special theory may be called a bound model; a special theory that is not based on a general one may be called a free model. See Vol. 5, Ch. 9, Sect. 1.2.) The difference is philosophically and didactically interesting for a number of reasons. For one thing the general theory cannot be obtained from the corresponding special theories by an inductive process: the general-special relation is deductive. (Yet some reputable scientists and philosophers have propagated the myth that continuum mechanics can be built out of particle mechanics.) Secondly, the more specialized a theory the better testable it is, because all test situations are particular. Moreover, all the tests of general theories are indirect, namely via special theories. (For example, the best tests of Newtonian mechanics are observations in planetary astronomy.) Thirdly, physics makes contact with engineering at the laboratory and on the level of special theories. The reason is clear: engineers are particularly interested in theories accounting for the physical aspects of artifacts – e.g. theories of the flow of liquids of certain types, or of directional antennas, or of nuclear reactors.

Finally we come to the *metatheoretical principles* in physics, such as those of correspondence, combined parity (CPT), and general covariance. (The adjective 'metaphysical' would be more precise but it has been preempted as a synonym of 'ontological'.) The metatheoretical principles are of course

about theories or some of their components, such as laws; when they refer to the latter they can be called 'metanomological'. (See details in Bunge 1961.) Let us examine them quickly.

The best known of them is the correspondence principle, which was employed explicitly in building the two relativities and the quantum theories. It states that the more general theory should reduce to the more special one in the domain in which the latter holds. For example, special relativity should (and in fact does) reduce to classical physics for small velocities; and in turn it should (and in fact does) hold as a good approximation of general relativity in flat spacetime. The principle of correspondence is heuristic during the stage of theory construction; once the new theory has been built, the principle is proved with paper and pencil: it has become a metatheorem. The methodological interest of the correspondence principle is obvious: it functions as a guide for building and evaluating new theories. The mere existence of principles of this kind refutes the fashionable opinions that "anything goes" in science, that theory choice is a purely subjective affair, or that it is governed exclusively by external (social) considerations. It also refutes the view that scientific revolutions raze the past: it shows that present knowledge guides the search for new knowledge, that the march of science is evolutionary. (See Vol. 6, Ch. 12, Sect. 3.1.)

The combined parity (CPT) theorem is a metatheorem that can be proved rigorously. It states that all local (nearby action) field theories (such as classical and quantum electrodynamics) are invariant under the simultaneous reversals of all charges, parity, and time. These reversals are conceptual, not factual. Thus, the theorem tells us that, if *we* invert the charge *sign* (on paper), then *we* must also invert the parity and time *signs* (on paper) if we wish our basic field *equations* to remain invariant. Clearly, then, the theorem, unlike a conservation theorem, say, does not represent a law of nature: it is a metanomological formula. Hence it cannot be subjected to experimental tests except indirectly, i.e. through the test of some other consequences of the theory concerned. Its main philosophical interest lies perhaps in suggesting that, in principle, it is possible to build one rigorous metatheory (however small) for any given physical theory.

As for the general covariance principle, it states that the basic formulas (though not all of their consequences) of a fundamental theory ought to remain unchanged under general linear coordinate changes and, in particular, under coordinate transformations representing exchanges of reference frames (hence in particular of observers). The theories that are in fact generally covariant are naturally called 'general relativistic'. Those covariant

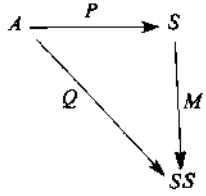
under more special coordinate transformations receive special names, such as 'Lorentz covariant' or 'Galileo covariant'. Contemporary continuum mechanics contains a similar principle that refers not to the fundamental equations of motion or field equations, but to the constitutive equations, which describe constant relations among properties of materials of special kinds. The principle, called of 'material frame indifference' or of 'material objectivity', states that the constitutive equations must be invariant under changes of reference frame (a fortiori of observer).

These various covariance principles are philosophically interesting for various reasons. First, they are not laws of nature but *principles of theory construction which become metatheorems* once the new theory has been built. Second, although some of them are heuristically powerful, none has deductive power (i.e. nothing follows deductively from any of them). Hence they should not occur among the axioms of a theory – *pace* a number of distinguished authors. Third, they show that, although some of the individual properties of things are relative or frame-dependent, the basic laws are not: the latter are absolute or coordinate-free. A fortiori, these laws are invariant with respect to changes in observer, i.e. they are the same for all observers. Therefore the covariance principles constitute a pillar of scientific realism.

Note that physics contains metatheoretical principles of two kinds: theorems and rules. The Lorentz covariance of Maxwell's equations and the CPT invariance of local quantum field theories are mathematical truths and, more particularly, metatheorems belonging to the metatheories of the corresponding physical theories. On the other hand the correspondence principle, the covariance principles, and the quantum-mechanical rule according to which every "observable" (dynamical variable) ought to be represented by a hermitian operator, are heuristic (involved in theory construction) or methodological rules or norms. Being different they should be evaluated differently: the theorems by canons of mathematical rigor, the rules by the value of the theories they help build. Still, we have also seen that the theorem/rule distinction is historically relative, in the sense that what begins as a heuristic rule may end up by being proved as a metatheorem.

The metatheoretical principles are metastatements not object statements. Does this entail that they lack any factual reference or content? This is what Kretschmann (1917) held with regard to general covariance, though on different grounds. We proceed to prove that the metatheoretical principles of physics referring immediately to law statements refer mediately to the physical objects referred to directly by the latter; so, they do have a factual content after all. The proof employs the principle of our theory of reference

(Vol. 1, Ch. 2) according to which the reference class of a predicate equals the union of the cartesian factors of the domain of the predicate. Let P be an object predicate, i.e. one representing a property of concrete objects of some kind. For the sake of simplicity let us assume that P is a nondimensional magnitude representing an intrinsic (absolute) property of things of kind A. In short, assume that $P: A \to S$, where S is the set of statements containing P. Consider next a metapredicate M, such as "is generally covariant", or "is true". M may be construed, similarly, as a function $M: S \to SS$ from statements S to metastatements SS (containing M). Finally form the composition of the functions P and M in this order. I.e. build the function $Q = M \circ P: A \to SS$ shown in the commutative diagram



Obviously, the new predicate Q is ultimately about A's. In other words, the manifest or *immediate* referent of a metanomological statement is a law statement; but its deep or *mediate* referent is the same as that of the corresponding law statement. *Example* Law statement: "The Angular momentum of a body in a central force field is constant". Metastatement: "The preceding statement is translation invariant". Composition: "The constancy of the angular momentum of a body in a central force field does not alter under translation". This rigorous result solves Kretschmann's problem, discussed endlessly in the literature.

2. Two classics

2.1. *Classical Mechanics*

Just as arithmetic used to be regarded as the queen of mathematics, so mechanics was the queen of physics until the birth of field theories *ca.* 1850. Mechanics is not only the first comprehensive and successful factual theory to be proposed in history: it is also the one dealing with the most conspicuous of all processes, namely the change of place of perceptible bodies. No wonder then that mechanics is presupposed by other physical theories and that it has attracted the attention of so many eminent mathematicians and philosophers. In fact, analysis was originally introduced, in part, to model

motion, and classical mechanics is the daughter of ancient atomism – the first mechanistic philosophy – as well as the mother of modern mechanism. Yet, despite its antiquity, research in classical mechanics is still producing novelties: see the *Archive of Rational Mechanics and Analysis*. And the philosophy of mechanics is still in its infancy. Here we shall be able to touch on just a handful of philosophical and foundational problems in classical mechanics. (See Truesdell 1984 for more.)

To begin with, let us be clear about the objects or referents of classical mechanics: they are bodies, i.e. physical objects endowed with mass and precise (though often complex and variable) geometrical properties, such as a definite shape. Massless objects, such as photons and neutrinos, are studied by field theories; and shapeless objects, such as electrons, are studied by quantum "mechanics". Mechanics handles forces in a phenomenological manner, i.e. without assuming any field mechanisms. Consequently mechanics can describe a body as a whole and even its state of internal stress. But it cannot account for the very existence of bodies, all of which are composed by atoms held together by fields of various kinds, in particular electric fields.

In the preceding paragraph we have inadvertently and tacitly rejected two myths that are rather popular among physicists and philosophers. One of them is the belief that the mechanics of extended bodies (i.e. continuum mechanics) can somehow be reduced to the mechanics of point particles. This is of course impossible: a continuum cannot be built up from separate points. On the contrary, the mass point results from specializing the density function to a sharply peaked one (a delta). In other words, particle mechanics is a subtheory of continuum mechanics, not the basis of the latter. The second myth is that classical mechanics underlies all other theories of massive entities, in particular quantum mechanics. (This myth originates in another, namely the operationist dogma that quantum mechanics refers exclusively to experimental situations, all of which are supposed to be describable in classical terms. More on this later.) This too is a mistake, because (*a*) only quantum mechanics is in a position to explain the formation and properties of extended bodies out of smaller entities (which are not classical particles), and (*b*) it is in several ways incompatible with classical mechanics.

We have stated that mechanics is a factual science because it accounts for bodies. This is obvious from a semantic analysis of any of the predicates of mechanics, such as the concepts of mass and position of a body. However, from Kant down to the dawn of our century it was usually

assumed that mechanics was a part of mathematics: that its principles were truths of reason, perhaps even necessary ones, rather than fallible hypotheses. (See e.g. Whewell 1847, Part I, p. 248.) The conventionalists went so far as to state that the axioms of mechanics were disguised definitions. Everybody called *rational mechanics* what we now call 'classical mechanics' – and all mathematics students were required to study it.

There were several reasons for the belief that mechanics is an a priori science like mathematics. One is that, from a purely formal viewpoint, every formula of mechanics is a formula of analysis: in this regard mechanics is, indeed, only a special case of mathematics. (But, of course, no formula of analysis is in fact one of mechanics unless it be interpreted in mechanical terms. Recall from Ch. 1, Sect. 4.1 the need to enrich a mathematical formalism with semantic assumptions in order to generate a factual theory.) Another reason for the belief we are commenting on was the apparent perfect fit of mechanics to analysis – which should not have been surprising given that the calculus had been invented largely as "the mathematics of motion", to the point that functions were often "defined" as the trajectories of moving points. A third root for the mistake is that, as in the case of other fundamental theories, most of the evidence for mechanics came after it was built. This too is understandable, since the very design and interpretation of any experiment involves some knowledge of mechanics. In any event, the advent of the special theory of relativity (1905) demolished the aprioristic thesis by showing that the laws of classical mechanics hold only for slow motions: they are not synthetic a priori truths.

From Kant till the birth of general relativity (1916) it was commonly believed that at least one ingredient of classical mechanics (and most other physical theories as well) was *both* factual (or synthetic) and a priori, namely geometry. This belief was perfectly reasonable in view of (*a*) the excellent fit of Euclidean geometry to physical space (in the small) and (*b*) the familiarity with the procedure of mathematizing particles as points, lines of flow as curves, bodies as regions of space, and so on. It was eventually realized that this procedure is just the dual of the procedure of interpreting geometrical objects as physical (in particular mechanical) ones. Nowadays we all understand what Gauss had suspected in 1821: namely that there is no identity between mathematical and physical geometries, but that a few geometries can be made to represent the spatial properties of physical objects and their relations. Hence, when axiomatizing classical mechanics we place Euclidean geometry in the formal background (or bag of presuppositions) of the former, and press it into service every time we need to

describe the spatial properties of bodies – e.g. the boundary of a body or the shape of a flow line.

Although it is now generally understood that a physical geometry results from enriching a mathematical geometry with a set of semantic assumptions ("correspondence rules"), not all aspects of the question are clear. One of them is this: What view concerning the nature of space is classical mechanics committed to? In other words, what ontological geometry does classical mechanics presuppose? It may be remembered that there are two families of such geometries: the absolutistic and the relational ones. (Vol. 3, Ch. 6, Sect. 1.1.) According to the former space is self-existing (absolute), whereas according to the latter space is the basic network of relations among physical objects, so that space would cease to exist if it were possible for the world to be annihilated.

Now, it is usually believed that, just because Newton happened to favor absolute space and time, classical mechanics presupposes an absolutistic theory of space and time. This is mistaken: classical mechanics is consistent with either ontological geometry. (Moral: The history of science can be misleading unless accompanied by the foundations and philosophy of science.) In fact the distance functions occurring in classical mechanics are defined on pairs of spatial points that may or may not be occupied by particles; and time may but need not be regarded as a function defined on pairs of events. (See Noll, 1967.) Thus it makes no difference to mechanics whether its independent variables x, y, z, and t are regarded as real numbers taken in themselves or as values of one or more (coordinate) functions defined on domains containing sets composed of physical objects such as things or events. (In particular, the equations of motion remain the same whether t is regarded as an arbitrary real number, as the parameter of a certain continuous group of transformations, or as an arbitrary value of a time function such as, e.g., $T\colon E \times E \times F \times U_T \to \mathbb{R}$, where E is a set of events, F a collection of reference frames, and U_T the set of all possible time units.)

In short, it is not true that classical mechanics is wedded to absolute space and time: in fact it is neutral in the conflict between the absolutistic and the relational views of space and time. On the other hand experimental mechanics and, in general, experimental physics adopts unwittingly the relational view, for we can measure only distances between physical entities, and durations between events. So, if we wish theoretical physics to match experimental physics, we must adopt a relational ontological geochronometry. However, this will show only in general relativity (Sect. 3.2). Mecha-

nics is quite insensitive to the choice between the two views on the nature of space and time.

A related topic is that of the nature of reference frames. With but a few exceptions physicists identify reference frames with coordinate systems. However, a coordinate system is a mathematical concept and therefore an object that cannot move relative to anything; on the other hand reference systems are *physical* systems that can move relative to other frames. For this reason the more careful authors state that coordinate systems map or represent (actual or possible) reference systems, which are distinguished physical systems such as nearly rigid bodies, possibly with clocks attached to them. (See Bunge 1967c.) In any event, whenever solving a particular problem in any theory containing the concepts of space and time, we must adopt both a physical reference system (e.g. a laboratory or the sun) *and* a coordinate system of some kind (e.g. cartesian or cylindrical) representing the latter.

What holds for coordinate systems holds for coordinate transformations. We must distinguish the purely mathematical relation between two different coordinate systems from a frame transformation such as a galilean one. The former relates the coordinate systems, e.g. the translation combined with dilation summarized in the formula $\ulcorner x' = ax + b \urcorner$, where a and b are real numbers. On the other hand the galilean frame transformation $\ulcorner x' = x - vt \urcorner$ relates the positions of a *physical* object relative to two different *frames* moving relative to one another at speed v, after the duration t has elapsed. Likewise the Lorentz and the general relativistic transformations concern reference frames not just coordinate systems.

The notions of reference frame and frame transformation are central to mechanics because all positions, and a fortiori all motions, are frame dependent. The relativity of motion is obvious from the fact that it can be transformed away (at least locally) by adopting a comoving reference frame. Of course, any such "rest description of motion" proves only that motion is relative, not that it is illusory – the more so since any object at rest relative to some frame moves relative to other frames.

Reference frames are so important that they are usually involved in the very formulation of the basic laws of motion. In fact, the correct formulation of these laws is as follows: "There exists (really) at least one reference frame relative to which the following equations hold [and here the equations are written]." Once the laws have been stated we can introduce the definition of a particularly important type of reference frame: "Any reference frame relative to which the preceding laws statements hold is called an *inertial*

frame". Since different theories are characterized by different equations of motion, there are different types of inertial frame: more on this in Sect. 3.1.

Just as we distinguish coordinate systems from reference frames, we must not confuse the latter with observers – another common confusion in the physical and philosophical literature. We must avoid this confusion for the following reasons. Firstly, an observer is only a very special kind of reference frame. Moreover, when attached to Terra observers are not inertial frames, hence they do not serve as referents (together with bodies) of the basic laws of mechanics. Secondly, even if observers were adequate reference frames on Earth, it is unrealistic to assume that they could live everywhere in the universe. In any case most reference systems used in physics (including astronomy) are uninhabited. Thirdly, the confusion between reference frame and observer invites subjectivistic misinterpretations of physics. Thus the true statement "All wavelengths are frame-dependent" can be misinterpreted as "All wavelengths are observer-dependent, i.e. subjective, i.e. physically unreal". More on this in Sect. 3.1.

The distinctions between reference frame on the one hand, and coordinate system and observer on the other, are crucial in the formulation and discussion of the relativity principle. There are several inequivalent versions of this principle. The most popular ones concern the alleged invariance of phenomena, or even experiments, as viewed by different observers. However, not all facts are the same in (relative to) all frames of reference. For example, the Doppler effect does not occur at the light or sound source. (Nor do law statements remain formally invariant when rewritten in different coordinates; for example, when translating a wave equation from cartesian to spherical coordinates additional terms appear.) The correct version of the relativity principle is about laws, not facts, and about reference frames, not observers. It reads thus: ⌜The *basic* law statements *ought to be* the same regardless of the reference frame⌝. This is, of course, a normative metanomological statement (Sect. 1.2.).

Let us now turn to somewhat more substantial issues, in the first place the notions of mass and force. Both have been the object of much philosophical speculation since the time of Newton. They were even the pillars of the *Kraft und Stoff* materialism popular in the 1850s. Mach and other anti-materialists tried to rid physics of the two concepts by defining them. (Mass was "defined" as relative acceleration, and force as the product of mass by acceleration.) However, we now know that such "definitions" are circular because they presuppose Newton's laws of motion, which contain the mass and force concepts. (See Bunge, 1966.) The concept of mass,

which seemed obscure and suspect one century ago, is nowadays construed exactly in the same way as Newton did, namely as the quantity of matter of the kind that mechanics deals with. Thus the atomic mass of an atom is roughly equal to the number of nucleons contained in its nucleus.

So, if anything, the concept of mass is even more deeply ingrained in physics than before Mach's criticism of it. The same holds for the concept of force: instead of eliminating it, physics has multiplied (and inter-related) the number of types of force – or, more precisely, of fields. True, it is mathematically convenient to describe fields in terms of potentials rather than forces. However, given a potential the corresponding force is definable – in the simplest case – as the gradient of the potential. True, static potentials are characterizable in purely geometric terms. But in general they are also time dependent. True, for operationists and conventionalists fields are just fictions convenient to summarize certain empirical data. But we know this thesis to be false for the following reasons: (*a*) fields can propagate at measurable speeds and they can interact with chunks of massive matter, such as electrons; (*b*) all the field "quantities" (e.g. the potentials) refer to putatively real fields, this being why they are studied in physics rather than in mathematics; (*c*) sometimes fields (and their equations) are postulated, and only later on experiments to test such hypotheses are devised with the help of the corresponding field theory.

Let us now turn to a couple of metatheoretical problems. But before doing so we must characterize the notion of a well posed mechanical problem, because some metatheoretical questions are pseudoproblems arising from overlooking some of the ingredients of a *well posed* mechanical problem. These ingredients are:

– the equations of motion (or the variational principles entailing them),
– the force laws to be plugged into the equations of motion,
– the constitutive equations characterizing the particular materials concerned,
– the constraints on the position coordinates,
– the empirical data (or corresponding prescriptions): mass density (or total mass as the case may be), stress tensor, initial conditions (initial values of positions and velocities), and boundary conditions (values of certain properties on the body border).

The above items are also necessary for posing correctly certain metatheoretical problems. One of them is whether classical mechanics is time-reversible, i.e. invariant with respect to the change in the sign of t. The usual answer is affirmative because it is limited to examining the equations of

motion, which, indeed, are T-invariant because the acceleration is, and mass and charge are tacitly assumed to be T-invariant as well. But this reasoning is incorrect because it overlooks the other ingredients of the problem, in particular the constraints, boundary conditions, and constitutive equations, which restrict the realistic solutions to the equations of motion. The elementary example of Figure 2.2 suffices to refute the common belief that, unlike quantum mechanics, classical mechanics is time-reversible, i.e. T-invariant (Truesdell 1974, 1982a).

A similar example of motion with impact can be used to explode the even more popular myth that classical mechanics is the paragon of a "deterministic" theory: Figure 2.2(c). A mass point strikes a wedge and is deflected either to the left or to the right. In this case there is no unique solution, so it is impossible to make an unambiguous prediction from knowledge of the equations of motion and the initial conditions. (See Truesdell 1974.) The problem calls for the assistance of probability theory. A quickly growing body of probabilistic classical mechanics (see Axelrad 1983) is the best epitaph to the myth that classical mechanics is inherently "deterministic" (non-probabilistic). What characterizes classical mechanics is not so much that it fails to contain probabilistic concepts, but that it conceives of bodies as possessing definite shapes, positions, and trajectories – hence also sharply defined velocities, angular momenta, and energies. This assumption is relaxed in quantum mechanics: See Sect. 4.

Even more striking illustrations of "indeterministic" and therefore un-

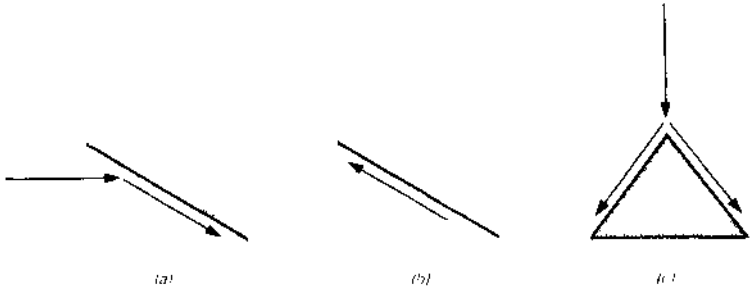


Fig. 2.2. A motion that is not invariant under time reversal, i.e. under the reversal of the velocity (and spin if any). (a) Original motion with impact; (b) reversed motion. In this case reversibility is obtained by requiring that the velocity (not just the trajectory) be continuous (Clifford Truesdell, personal communication, 1982). (c) A motion with indeterminate outcome: the incoming path bisects the wedge angle (Truesdell, 1974).

predictable behavior occur in non-linear classical mechanics. If the equations of motion are non-linear then the steady state orbit of the system (its "attractor") may be "chaotic" (irregular), hence unpredictable. The motions on such an attractor *look* chaotic although they satisfy ("are ruled or controlled by") nonprobabilistic ("deterministic") equations of motion. Such orbits are characteristically unstable in the sense that a minute change in the initial conditions may result in a large difference in the orbit; moreover bifurcations emerge, so that the position coordinate may take almost any value. (A well-studied example is the finite difference equation $\ulcorner x_{n+1} = \lambda x_n(1 - x_n)\urcorner$, where n is a natural number, and x ranges between 0 and 1, whereas λ can vary between 0 and 4. For a given value of λ there is a bifurcation to two different steady state solutions. As λ increases, each of these solutions branches out into another two, and so on. For a certain critical value of λ bifurcation occurs infinitely many times, i.e. x can assume any value. See e.g. Hellman, Ed. 1980.) Morals: (*a*) randomness can be simulated by perfectly "deterministic" systems, hence it cannot be diagnosed from a mere inspection of data such as observed trajectories; (*b*) determinism, an ontological category, cannot be defined in terms of predictability, an epistemological one (Bunge 1959a).

This subject dovetails with that of the next subsection.

2.2. *Statistical Mechanics*

Classical mechanics deals with bodies and systems of such. The links among the system components, such as contact and gravitational forces, generate and preserve the system itself, as each component codetermines the behavior of every other component. As soon as these links weaken, the components acquire some leeway, as a consequence of which some measure of randomness is bound to creep into the system. Thus the phases (or initial displacements) of an aggregate of mutually independent (or nearly independent) oscillators are bound to be distributed at random.

Classical mechanics is incompetent to handle systems composed of huge numbers of weakly interacting things – such as gases – except as black boxes, i.e. by deliberately ignoring their composition and structure. The study of systems composed of a very large number of units (typically 10^{27}) linked by weak forces, and a fortiori systems of mutually independent components, belongs to statistical mechanics. This theory assumes classical mechanics and it contains randomness hypotheses, such as probability distributions of initial positions and velocities. Hence it uses not only the mathematical tools of mechanics but also the probability calculus. In other

words, statistical mechanics results from the merger of mechanics and certain probabilitistic hypotheses.

Determinists of the classical type regard this merger as artificial and necessitated by our ignorance of microscopic details rather than by the nature of things. In other words, they regard chance as a synonym of ignorance, hence as an epistemological category rather than an ontological one. They argue that if only we could get to know the initial position and velocity of every single atom in a gas or a liquid, we would not need the probability concept. (Put in theological terms: God does not need probability nor, a fortiori, statistical mechanics.)

This argument overlooks the fact that the independence (or near independence) of the components of a system such as a gaseous or liquid body is a source of objective chance or randomness (not of chaos or lawlessness). According to classical statistical mechanics chance is neither in the individual system components nor in the eyes of the (blind) beholder, but in the nature of things: it is an *emergent property* on the same footing as other system properties such as entropy and temperature. (Recall the definitions of emergence in Vol. 3, Ch. 2, Sect. 4.3, and Vol. 4, Ch. 1, Sect. 3.2.) The simplest randomness (or chance) hypotheses in statistical mechanics concern the initial positions and velocities of the system components. But these are not the only ones. Thus the modeling of the crystallization process in a liquid calls for probabilistic hypotheses concerning the nucleation process. Since these events are random in space and time, neither the place nor the time of the emergence of the first crystal can be predicted exactly.

A key probabilistic concept of statistical mechanics is that of thermodynamic probability, or number of microstates (of the components) compatible with a given macrostate (of the system). This is an objective property of the system, namely a measure of the play, leeway, or "degeneracy" of the system components. It is an important property because it is used to calculate another system property, namely its entropy. In fact, the simplest measure of the entropy of a system makes it proportional to the logarithm of the thermodynamic probability of the system.

The thesis that thermodynamic probabilities are *objective* system properties is confirmed by the fact that they can be measured in many cases. (In the case of quasistatic changes one measures temperatures and quantities of heat, and calculates the entropy by means of the second law of thermodynamics.) But if one interprets probability in a subjectivist manner – e.g. as a measure of one's information, or rational belief – then the entropy must be interpreted in the same manner. That is, entropy ceases to be an objective

property of multicomponent systems, to become a measure of some knowing subject's uncertainty concerning the precise microphysical configuration of the system. (As one physicist put it, entropy measures "our difficulty of deciding what state the system is in".) This subjectivist interpretation turns the law of increasing entropy (or 2nd law of thermodynamics) into a law of the psychology of knowledge. Obviously, if physics is to remain an objective science – pardon the redundancy – then one will speak of the play (or "degeneracy") of microstates compatible with a given macrostate, instead of the subjective probability concerning our knowledge of such states. (More on subjective probability in Ch. 1, Sect. 4.2.)

Yet Jaynes (1979) appears to have reformulated classical statistical mechanics using the subjectivist concept of probability (as a measure of the plausibility of a hypothesis) and a subjectivist interpretation of information theory (as measuring the decrease in one's ignorance upon receiving a message). How has it been possible for him to obtain correct general results, even new ones, with such obviously incorrect interpretations? There are several reasons for this, among them the following. Firstly, the final formulas in statistical mechanics do not involve probabilities of elementary events but only macrophysical magnitudes, in particular averages; so, no subjectivist assumptions occur in them, Secondly, Jaynes has simplified matters by assigning uniform probabilities to the macrophysically irrelevant details; so, the estimates of such probabilities are not a matter of subjective judgment after all. Thirdly and most important, Jaynes's subjectivism is quite limited, for he does not claim that a thermodynamic system possesses an entropy only because we do not know (or do not care to find out) its microphysical details. Rather, he states that when using an entropy function we deliberately disregard such details. In fact he writes explicitly: "The entropy of a thermodynamic system is a measure of the degree of ignorance of a person *whose sole knowledge about its microstate consists of the values of the macroscopic quantities* X_i which define its thermodynamic state. This is a completely 'objective' quantity, in the sense that it is a function only of the X_i, and does not depend on anybody's personality. There is then no reason why it cannot be measured in the laboratory" (Jaynes, 1979, p. 42). Clearly, then, the interpretation of entropy as a measure of the degree of ignorance is an *idle assumption* that does not occur in the calculation or in the interpretation of the final results. In conclusion, Jaynes's version of statistical mechanics is just as objective as the standard one. (There are two further reasons, of a technical nature, for Jaynes's success. One is that he does not confuse probabilities with frequencies, so he has not been slowed down by having

to imagine impossible experiments. The other is that he has refrained from using the controvertible ergodic hypothesis.)

So far we have been concerned tacitly with what is usually called the Boltzmann model of a complex system with more or less random components. There is an alternative model (or method) that at first sight is neither quite as "indeterministic" nor as realistic as Bolztmann's, namely Gibbs's. The Gibbs model refers not to a single system but to a whole *imaginary* ensemble of similar copies of one and the same system. Each copy differs from the others in some microphysical detail. Each copy is supposed to behave "deterministically", but the price for this apparent gain is that the probability distributions are defined over the imaginary ensemble (or collection of copies). Nor is the Gibbs model any less realistic than the Boltzmann one, for it postulates that the real aggregate behaves like the average over the given imaginary ensemble. Thus the two models represent differently one and the same real macrosystem, and both are probabilistic.

Whether in the Boltzmann or in the Gibbs formulation, statistical mechanics deals with both reversible and irreversible processes. A reversible process is one that takes the system back to its initial state without any extra energy expenditure. A convenient way of representing the reverse of a process is to invert the sign of the time variable or, what is equivalent, to reverse the velocities of all the components. For a process to be reversible it is necessary, though not sufficient, for its laws to be time-reversible (or T-invariant). We saw in Sect. 2.1 that T-invariance is only necessary for motion to be reversible. Another example is the propagation of a spherical electromagnetic wave: unless the wave is generated at the center of a perfectly reflecting spherical surface, there is no way it will contract to a point.

Statistical mechanics associates irreversibility with entropy increase without identifying them. Irreversibility is necessary but insufficient for entropy increase. In fact there are irreversible processes which are not thermodynamical. For instance, introducing a coin into a piggy bank is far easier and requires less energy than taking it out. The reason is that in the former case we control the coin and therefore the boundary conditions, whereas in the latter case we control only the system as a whole, and so must rely either on blind shaking or on brute force. Another example is the transmutation of an electron-positron pair into three gamma photons. This is usually described as a reversible process. (Moreover all elementary processes are said to be reversible.) But it is not, for the chance of three photons meeting at a given place is very slight.

Statistical mechanics was originally invented to explain thermodynamics, the paragon of a phenomenological or black box theory. Thus, statistical mechanics explicates the total energy of a system as the sum of the kinetic and potential energies of its components, and the entropy of the system as the "degeneracy" or leeway of its microstates relative to its macrostates. Thermodynamics is probably the only important physical theory to have been born in technology – but of course raised in mathematics. This explains why its principles are still often couched in terms of thermal machine cycles, or even in anthropocentric terms. Thus its most famous (yet still intriguing) principle – the second – is sometimes stated thus: "The energy of an isolated system is forever degrading into less useful forms". A more sober formulation of this principle is this: "The total entropy of an isolated system does not decrease in the course of time". (This holds only in the long run: there are temporary random fluctuations. I.e., the 2nd law is probabilistic.)

Since its inception the second principle of thermodynamics has been used to buttress the eschatological view that everything in the world is doomed to ultimate decay, to the point that the entire universe is bound to end up in a state of "thermal death" or maximum entropy. This would actually be the case if the universe were finite. But there is no firm cosmological evidence for this assumption; therefore we had better suspend judgment on the total entropy of the universe. Besides, even assuming that the universe were a closed system and therefore subject to the 2nd law, nothing would prevent it from containing subsystems the total entropy of which were constant or even in the process of decreasing. In fact this is what happens in the formation of stars, star clusters, crystals, organisms, etc. We shall return to this problem in Sect. 8.2.

Another popular myth is the belief that, since everything is fated to run downhill, the emergence of biosystems violates thermodynamics and calls for nonphysical, perhaps even theological, principles. This is a gross mistake, for we all know that biosystems are open systems and therefore not subject to the 2nd law. Every time a protein molecule is synthesized, or a cell divides, a negentropic process occurs. The organism as a whole maintains a constant level of entropy at the expense of its environment. The 2nd principle does not apply to the organism but to the supersystem composed by the organism and its environment. (For the emergence of "structure" in open systems far from equilibrium see e.g. Glansdorff and Prigogine 1971.)

Statistical mechanics was invented to explain thermodynamics, but in fact it has so far failed to do so: the reduction is a textbook myth for the

edification of undergraduates and philosophers. What has been reduced is only (*a*) the kinetic theory of ideal gases (summarized in the constitutive equation $\ulcorner pV = nRT \urcorner$), and (*b*) a portion of thermo*statics*, which is the thermodynamics of systems in equilibrium. In particular there seems to be no *general and rigorous* derivation of the second law of thermodynamics, much less of the regularities of phase transitions (such as freezing). Typically, the rigorous treatments refer to spatially infinite (and sometimes one-dimensional) systems (Ruelle 1969, 1978), or to quasistatic (extremely slow) changes (Martin-Löf 1979), or to simple monoatomic gases or mixtures (Muncaster and Truesdell 1979). Even in these cases the results are restricted to the very special case of uniform density and temperature – a state in which macroprocesses, such as convection and heat transfer, do not occur. In short, the general "reductions" are sloppy, and the rigorous reductions concern only very simple and usually unrealistic cases. The general and rigorous reduction of thermodynamics to statistical mechanics is not a fact but a program (Bunge 1973a p. 190, Brush 1983 p. 261). Nor is it reasonable to expect that a full reduction is *possible*: after all, the components of a thermodynamics system are quantum-mechanical things not classical ones. The reduction, if possible, will be to quantum statistical mechanics.

(Boltzmann and Planck are usually credited with having surmounted the most severe difficulty in reducing thermodynamics to statistical mechanics, by proving the 2nd law. Actually what they did was this: (*a*) to *postulate*, not deduce, that an isolated system ends up by having its components distributed in the most homogeneous manner possible compatible with the external constraints (container shape, fields, etc.); (*b*) to *prove* that such distribution (state) is the one that can be reached in the greatest number of ways – i.e. the one characterized by maximal thermodynamic probability; (*c*) to *prove* that in any other state the entropy is less than maximal; (*d*) to *identify* the logarithm of the thermodynamic probability with the entropy of the system, thus providing a bridge (definition, not law) between thermodynamics and statistical mechanics; and (*e*) to *suggest* without proof that any state with less than maximum entropy is earlier than the latter – i.e. that entropy increases except for small random fluctuations. Unfortunately this monumental work does not add up to a general and rigorous proof of the 2nd law, let alone of the deduction of thermodynamics from statistical mechanics – except in the imagination of elementary textbook authors. For more on this subject see Krüger 1976, Brush 1983, Truesdell, 1984.)

We close this section with comments on a couple of common mistakes. One of them is the extrapolation of the specific concepts of thermodynamics

to microphysics. Thus it has become commonplace to speak of the temperature and entropy of systems of spinning "particles" such as atomic nuclei (Ramsey 1956). The price paid for this extrapolation is quite high: (*a*) one and the same metallic sample is attributed two different temperatures near the absolute zero of temperature: those of the atomic nuclei and of the rest; (*b*) negative absolute "temperatures" are introduced, which are interpreted as indicating that a system possessing any of them, far from being supercooled, is "transfinitely hot" – whatever this may mean. Similar extrapolations, at comparable prices, might be conconcted – e.g. attributing vorticity and refractive index to electrons. Moral: we must recognize the emergence of new properties and the concomitant formation of new levels. This recognition calls for the introduction of some specific concepts representing the emergent properties. In other words, we cannot expect microthings to be accounted for by macrophysical theories, or macrothings to be accounted for by microphysical theories. All we can hope for is interconnecting such theories and partially reducing some of them to others. (For the concept of partial reduction see Vol. 6, Ch. 10, Sect. 3.1.)

Our second comment refers to a philosophical distortion of thermodynamics. Because temperatures can be *measured* only in conditions of thermal equilibrium, operationists have concluded that they can be *defined* only for systems in equilibrium, so that it makes no sense to attribute temperatures to systems in states far from equilibrium, such as a rapidly cooling body. This is like claiming that, since the weight of a body can be measured only when the body is at rest on a scale, moving bodies are weightless. If physicists had admitted that operationist restriction, thermo*dynamics* would not exist, and we would know only thermostatics. In particular, the study of heat transfer, e.g. along a metal bar, would have to be proscribed, and Fourier's law declared meaningless. This is not a joke: it may well be that the operationist's concern for measurement – indispensable in the laboratory but out of place in connection with general theories – has effectively blocked the progress of thermodynamics by concentrating the attention of physicists on equilibrium states and reversible processes. (See Giles 1964 for a gallant but, alas, unsuccessful attempt to construct thermodynamics out of elementary experiences. And see Truesdell 1982b for "the disastrous effects of experiment upon the early development of thermodynamics".)

3. Two Relativities

3.1. *Special Relativity*

Few scientific theories have been as badly misinterpreted as special relativity (SR). Let us examine briefly some of the most popular misinterpretations. (See Bunge 1967c and 1979b for more.) The earliest misinterpretation of SR was that it supports philosophical relativism, or subjectivism, for showing that a number of important physical properties, such as duration, length, and mass, are frame-dependent instead of being absolute as postulated in classical physics. The misinterpretation results from mistaking reference frames for observers (knowing subjects). Hence expressions such as 'apparent length', 'event x as seen by observer y', and even 'process x as seen by atom y', are still current in the physical literature. But reference frames, which are even more conspicuous in SR than in classical mechanics, are not observers: they are physical systems (e.g. rigid bodies) representable by coordinate systems. (See Bunge 1967c.) Moreover the only reference frame known to be inhabited by competent observers, namely our own planet, is inadequate for formulating the basic laws because it is accelerated.

Reference frames must then be distinguished from observers as well as from coordinate systems. (Recall Sect. 2.1.) In turn, neither reference frames nor coordinate systems should be mistaken for measuring devices, let alone for observers. See Table 2.1. The conflation of two or more of these

TABLE 2.1. Four different concepts often confused in the literature.

Concept	Rough characterization	Studied in
Coordinate system	n-tuple of functions (or of their values) serving to locate or label points or events in a space	Mathematics
Reference system	Macrophysical system with preferred spatial directions and possibly a clock	Physics
Measurement apparatus	Macrophysical system designed to detect events of some kind and measure some of their properties	Physics
Observer	Sentient being trained to make scientific observations of some kind	Psychology, epistemology, methodology

four concepts may produce the illusion that a mathematical definition of a coordinate system characterizes a measurement apparatus, or that every theory containing frame-dependent quantities is subjectivistic.

In classical mechanics lengths, durations and masses are absolute, i.e. the same in (relative to) all reference frames. On the other hand in relativistic mechanics lengths, durations and masses are relative to the reference frame. Therefore every body has infinitely many sizes and masses, and every process has infinitely many durations: as many as possible inequivalent reference frames. However, not all properties are relative in SR: only some classical absolutes, such as duration and mass, have been relativized; others, such as electric charge and entropy, continue to be absolute (frame invariant). In return for the loss of certain absolutes, SR has introduced absolutes unknown to classical physics, such as proper time and spatiotemporal interval. On the whole, if we count both the fundamental (undefined) and the derived (defined) magnitudes, it turns out that relativistic physics has just as many absolutes as classical physics. Indeed both sets are denumerably infinite, and each of them splits into two disjoint subsets: that of relative and that of absolute magnitudes, both subsets being infinite. (Just think, say, of the time derivatives of a position coordinate, or of the powers of the proper time.) So, in the end SR is not more relativistic than classical physics – and neither supports subjectivism.

Another popular belief is that in SR there are no privileged reference frames. (This negative statement sometimes passes for the principle of relativity.) That is not true: in SR the frames relative to which Maxwell's electrodynamics hold *are* privileged over all others. We call them *Maxwellian* inertial frames. Two such frames are equivalent if an arbitrary light ray propagates in a vacuum along straight lines with the same constant speed relative to both frames. And any two such frames are related by a Lorentz transformation (which leaves Maxwell's electromagnetic equations unchanged).

The Maxwellian inertial frames are not the same as those relative to which the equations of motion of classical mechanics are supposed to hold. We call the latter *Galilean* inertial frames. Two such frames are equivalent if an arbitrary particle (not a light ray) moves freely along straight lines with the same constant speed relative to both frames. Any two such frames are related by a Galileo transformation (with leaves Newton's mechanical equations unchanged).

The concepts of Galilean and Maxwellian inertial reference frames enable us to state one *principle of relativity* for classical mechanics and another for

classical electrodynamics. Both can be compressed into a single formula thanks to the ambiguity of the word 'inertial frame': "The basic (or fundamental) laws of physics ought to be the same in (relative to) all inertial reference frames". In Sect. 1.1 we noted that the relativity (or covariance) principles refer directly to basic (not derived) law statements, and only indirectly to facts; and that they are normative or prescriptive rather than descriptive. Now we stress that there are two mutually incompatible relativity principles in classical physics: one for mechanics (hence indirectly for bodies), the other for electrodynamics (hence indirectly for fields). In other words, whereas classical mechanics satisfies the Galilean principle, electrodynamics satisfies the Maxwellian one. The two theories are then mutually incompatible. Hence they cannot be merged into a consistent theory accounting for the motion of bodies in a electromagnetic field.

Lorentz and Poincaré attempted to surmount this contradiction by keeping the equations of motion of classical mechanics and reinterpreting them in terms of absolute length contractions and time dilations caused by the motion of bodies through the aether at rest. Einstein understood that the situation called for radical surgery: it was necessary to choose between the two theories, or else reject them both. He opted for classical electrodynamics not only because it was the better confirmed of the two but also because he believed the world to be made up basically of fields. That is, Einstein assumed that classical mechanics was false and set himself the task of building a new mechanics compatible with classical electrodynamics, i.e. one the basic equations of which would hold relative to Maxwellian reference frames (i.e. be Lorentz covariant). Thus was SR born: from an examination of theories, not from an analysis of experiments. (For a refutation of the textbook myth that Einstein merely mathematized the negative experimental results of the Michelson and Morley measurements, see Shankland 1963 and Holton 1973.) The reformation of mechanics was only the first step, and one that is still not fully completed. In time the whole of classical physics will be reformed to comply with the requirement of Lorentz covariance.

What is SR about: what are its referents? Eminent physicists have claimed that it is about yardsticks and clocks, or even about measurements performed by observers moving relative to one another. This operationist interpretation of SR can be refuted in either of two ways: by subjecting SR to a semantic analysis or by axiomatizing it. In either case it is shown that the concepts of observer, measuring instrument, and measurement, fail to occur among the concepts of the basic theory. (See Bunge 1967c.) Moreover

the theory is needed to elucidate these concepts as well as to design measurements inconceivable on classical physics, such as those of time "dilation". The genuine referents of a relativistic theory are physical entities. Among these, reference frames and electromagnetic (in general, massless) fields occur, as is obvious from the conspicuous occurrence of the limit speed c. For example, relativistic thermodynamics refers, like classical thermodynamics, to complex material systems; but, in addition, it refers to reference frames and electromagnetic fields. And the totality of relativistic physics – which is the union of all Lorentz covariant theories – refers to physical entities in general.

Another popular belief is that SR has geometrized physics and therefore eliminated becoming. This belief seems to stem from Henri Bergson, it was popularized by Hermann Weyl (1950) and was adopted uncritically by a number of philosophers of science. It is adequately described in a science fiction novel: "All moments of time exist together. The world can be thought of as a map, not only spatially, but also with respect to time. The map stretches away both into the past and into the future. There is not such thing as "waiting" for the future. It is already there on the map" (F. and G. Hoyle 1963 p. v).

Now, it is true that the history of any thing can be displayed on a spacetime diagram. But this can also be done in classical physics, and in either case it may involve an extrapolation – to the past and the future – of the observed segment of the history. If anything, becoming is even more deeply entrenched in SR than in classical physics because, according to the former, there are no instantaneous actions at a distance: all processes take some time. (SR cannot even be stated without the notion of a signal propagating with the limiting velocity c; a signal is a process, not a static thing.) Nor is time in SR one more coordinate, hence a purely geometric object: time is merely being represented by the fourth coordinate. The welding of space and time in SR does not rob time of its privilege, for (*a*) the concepts of space and time are mutually independent (logically irreducible), and (*b*) the changes of state of physical systems are described by formulas wherein time (or proper time) occurs as an independent variable. Spacetime has a fibrous structure, for any point in it representing a physical entity has its own light cone associated with it. And every light cone effects (*a*) an absolute (frame-independent) separation between timelike and spacelike vectors, and (*b*) a relative (frame-dependent) separation between past and future. So, the events in any given thing are not given once and for all: they happen sequentially and are thus partially ordered. (Moreover, this partial order of

events implies the Lorentz group: Zeeman 1964. True, one can proceed the other way around and base SR on the group-theoretic properties of spacetime: Lévy-Leblond 1976. However, this procedure does not explain why one should adopt the relativistic spacetime metric. Worse, since it takes spacetime as existing by itself – i.e. absolutely – this procedure gives the impression that SR is a purely mathematical theory unconcerned with physical entities.)

The concept of becoming is related to those of determinacy and predictability. Positivists equate determinacy with predictability just because the latter is a symptom or indicator of the former. The equation is incorrect for the following reasons. First, determinacy is an ontological category whereas predictability is an epistemological one (Bunge 1959a Ch. 12). Event A may determine event B even though we may be unable to predict B from a knowledge of A. Second, an unpredictable event may determine a knowable one even if the chain between the two events is not stochastic. For example, Figure 2.3 shows event A, which influences or even causes event B, but lies outside the light cone of observer at O, who therefore cannot have knowledge of A, hence cannot predict B.

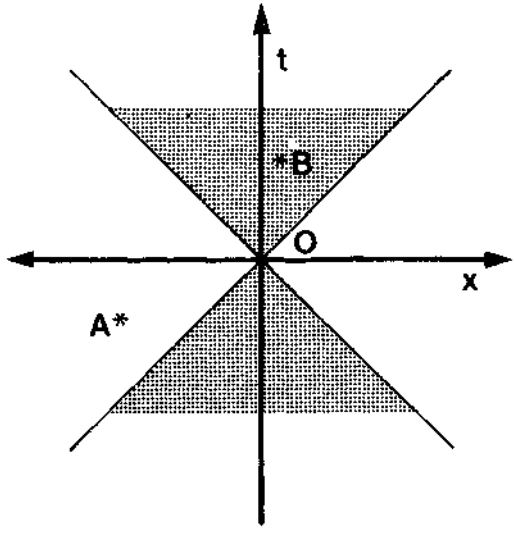


Fig. 2.3. Light emission at A, unknowable in principle to observer at O, influences event B, which the observer can perceive but not predict.

It is well known that SR has restricted the field of the causal relation to pairs of events connectible by light signals. What is far less well known is an even deeper conceptual modification introduced by SR in our causal thinking, namely the idea that *not every difference is an effect*, i.e. an outcome of a causal action. In fact the differences between the lengths (or durations, masses, temperatures, etc.) relative to mutually inequivalent reference frames are just differences, not effects of some events: they parallel the differences in kinetic energy relative to different frames. In particular the length "contractions" and time "dilations" are not caused by anything, although they were initially explained as effects of the aether pressure. (There is a constant output of ingenious non-relativistic mechanisms to explain such differences in causal terms. See e.g. Edmonds, 1978.) The relativistic explanation is simply that any given body has as many sizes as there are inequivalent reference frames: size is not an intrinsic property of bodies. Duration and the other variant properties are parallel: they are all relational properties. This is paradoxical only if one sticks to classical mechanics.

The last myth we wish to explode concerns the status of the simultaneity of distant events: is it fact or convention? Einstein himself, whether by a careless use of the words 'definition' and 'conventional' or under the influence of Poincaré's conventionalism, asserted initially that distant simultaneity is conventional: i.e. that, whether two distant events are judged to be simultaneous or not, is a matter of convention not of fact. Grünbaum (1973) has held that SR is *based* on such convention rather than on a set of postulates representing natural patterns – such as, e.g., the constancy (or frame independence) of the speed of light in a vacuum. The polemics unleashed by this thesis have filled too many pages over the past two decades, although the question can be decided in one minute.

In fact, in order to find out whether two distant events are simultaneous relative to a given reference frame we calculate or measure distance and travel times of light signals using one of the basic assumptions of SR. This is the hypothesis that light propagates in a vacuum, relative to any reference frame, at a constant speed c. Example: a photocell detects, at time t_2, photons emitted at time t_1 by two light flashes equidistant from the receptor and at a mutual distance d: see Figure 2.4. This is not a convention but a physical law. Moreover it presupposes a refutable hypothesis, namely that space is homogeneous and isotropic. It might well happen that the speed of light had local fluctuations, and that in some regions the speed in one direction differed from that in the opposite direction. (In this case the

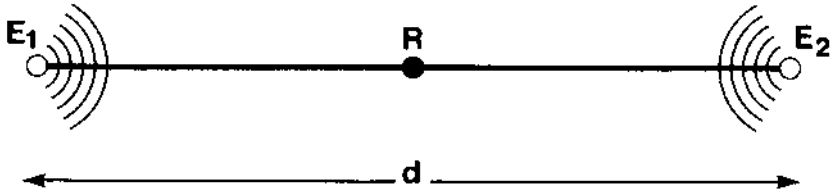


Fig. 2.4. Emitters E_1 and E_2, equidistant from receiver R, emit light pulses at time t_1 which receiver R detects at time t_2. The relation between emission and detection times is $t_1 = t_2 - d/2c$, where c is the speed of light. The simultaneity of the two emission and the two reception events, relative to the frame attached to R, is a fact, not a convention. The relation between the emission and detection times is a law not a definition.

Lorentz transformations would hold only on the average.) In short the simultaneity relation is frame dependent but not conventional: two events that are simultaneous relative to a given frame may cease to be simultaneous relative to a different frame. (In other words the simultaneity relation is ternary not binary.) And this is a trait of nature not a human convention. (More in Bunge 1968 and Torretti 1983.)

3.2. *General Relativity*

General relativity too poses a number of philosophical problems, in the first place that of the reference class of the theory. The operationist view is of course that general relativity is about observers engaged in measuring distances and durations. This statement is shown false by examining the basic gravitational field equations. These relate the gravitational potentials (represented by the space-time metric tensor) to the densities and stresses of the sources of the gravitational field, e.g. bodies and non-gravitational fields. (Briefly: $G = kT$.) General relativity refers then to matter in general and to the gravitational fields generated by matter. To be sure it also refers, in particular, to the special physical systems called 'reference frames'; but, as we saw in Sect. 2.1, frames are not to be mistaken for observers. And, sure enough, the theory may be applied to design measurements (of e.g. the sides and angles of an astronomical triangle formed by radio signals); but the theory is so general that it does not refer specifically to any measurements. In short, general relativity does not tally with operationism – as Bridgman (1949) himself admitted. This conclusion can be proved rigorously by applying our theory of reference (Vol. 1, Ch. 2) to the basic concepts of general relativity (as dug up in the axiomatization proposed by Bunge 1967c).

Another philosophy that fails to account for general relativity is *conventionalism*, which in this case boils down to the thesis that the space-time metric is conventional (Grünbaum 1973). One argument for this thesis would seem to be that the choice of geometrical coordinates is conventional: space-time has no coordinates stamped on it, these are constructs. True, but this holds for any theory containing the concepts of space and time. Space-time would in fact be metrically amorphous only if the assignment of spatio-temporal distances were arbitrary. But it is not, except with regard to the length and time units. On the contrary, the space-time interval between two events remains constant under a general coordinate transformation. In other words, it is perfectly objective. The line element in a Riemann four-manifold is given by $ds^2 = g_{ab}\,dx^a\,dx^b$, where $a, b = 0, 1, 2, 3$. When a general coordinate transformation is performed, the changes in the dx^a are compensated for by changes in the components of the metric tensor g_{ab} in such a manner that ds^2 remains unchanged. In other words, the value of ds^2 does not depend upon our particular choice of coordinates: it is *absolute*. This is best seen by using intrinsic (synthetic or coordinate-free) geometry.

A second argument against the conventionality (or amorphousness) of the space-time metric is this. The components g_{ab} of the metric tensor have a definite physical meaning in general relativity which they lack in the classical theory of gravitation: in fact they are the potentials of the gravitational field. (The field strengths are represented by the affinity definable in terms of the metric tensor.) A third argument is that the choice of coordinate system (hence of metric tensor) is not totally arbitrary but, on the contrary, it is restricted by the condition that the transformed time (space) coordinate be another time (space) coordinate, so that the conversion of time into space or conversely be precluded. More precisely, any coordinate systems that do not allow a distinction between space-like and time-like vectors, and all those coordinate transformations that do not preserve this dichotomy, are excluded explicitly by hypothesis. (There are further restrictions on the choice of coordinates.) Fourthly, it is true that the values of the components of the metric tensor at any point depend both upon the physical reference frame and the coordinate type. But this holds for all tensors (and vectors). What is important is that, once a reference frame and a coordinate system have been adopted, those values are not assigned arbitrarily or a priori. On the contrary, they result, via the field equations, from the distribution of bodies and non-gravitational fields in the vicinity of the point concerned. In other words, the geometric (or rather

gravitational) tensor G_{ab} is determined by the matter tensor T_{ab} according to the field equations $\ulcorner G_{ab} = kT_{ab} \urcorner$, where k is a dimensional constant. This point leads to two more questions of philosophical interest.

A first question is whether general relativity effects the geometrization of physics, as has often been claimed. If this claim were true, it would refute our dichotomy of the sciences into formal and factual (Ch. 1, Sect. 1.1). But it is not. The truth is not that the gravitational field has been *replaced* by a geometric object (the geometric tensor), but that the latter has been made to *represent* the former. More precisely, in general relativity gravity is represented by space-time curvature. Thus the metric of space-time, far from being a priori and independent of the furniture of the world, is a posteriori because it depends on the content of the universe. This is obvious from an examination of the basic field equations, which, as we saw a moment ago, refer to matter of all kinds and the associated gravitational fields.

A second question is this: What happens if the matter tensor T_{ab} occurring in the field equations vanishes everywhere and at all times? In this case the solutions of the resulting equations, namely $\ulcorner G_{ab} = 0 \urcorner$, still describe, in general, a curved (Riemannian) space-time. This would seem to contradict the statement that the gravitational field causes deviations of space-time with respect to the flat (or tangent) space-time. In fact there is no contradiction, for general relativity does not state that every deviation from flatness is due to gravitation, but the converse, namely that, if there is gravitation, then space-time is curved. So, space-time curvature is an *ambiguous indicator* of gravity.

It has been claimed that the existence of solutions of the field equations for a vanishing matter tensor countenances an absolute view of space-time, i.e. one according to which space-time is the self-existing container of every thing. It is trivially true that the equations $\ulcorner G_{ab} = 0 \urcorner$ define a *mathematical* space, or rather a family of spaces. But this does not prove that these spaces are physically meaningful, i.e. that physical space-time would continue to exist even in the absence of matter. To solve this problem we need more than mathematics: we also need a theory of reference, a precise notion of physical existence, and a methodology. We have supplied these tools in previous volumes of this Treatise; let us now apply them to the problem at hand.

If the matter tensor vanishes everywhere and at all times, then $G_{ab} = 0$, and there are no *physical* properties left either. Consequently the state function of the "universe" vanishes, and correspondingly its state space

shrinks to nothingness – which is precisely a mark of constructs, not of things. (Recall Ch. 1, Sect. 2.1.) In other words, space-time is in no physical state, and a fortiori it cannot jump to a different state: there are no pure spatiotemporal events, but only events occurring in physical entities such as atoms and stars. Again: nothing would ever happen in a hollow "universe" – a clear sign of its unphysical (mathematical) nature. The components of the metric tensor g_{ab}, which occur in the geometric tensor G_{ab}, cannot be interpreted as gravitational potentials in the absence of field sources for, in this case, there are no fields and consequently no gravitational potentials either. Consequently space-time cannot be adequately described by an absolutist theory consistent with general relativity. A relational theory of space-time is therefore called for. (See Vol. 3, Ch. 6 for one.)

There is, finally, the methodological problem of supplying evidence for an absolutist theory of space-time. Both theory and experiment tell us that it is impossible to shield space-time, as if it were a charged body, in order to find out what its intrinsic properties are. And common sense tells us that, since $\ulcorner T_{ab} = 0$ everywhere and at all times$\urcorner$ writes off all existents, it is incompatible with the existence of reference frames and measuring instruments. Even the weaker condition that the matter tensor vanish everywhere within a finite region of space-time is incompatible with the execution of space-time measurements in that region. If space-time were regarded as an absolute existent, rather than as a network of relations among things, its interactions with material entities, in particular measuring instruments, would have to be examined. But in this case one would not know how to proceed. In fact, a thing b acts upon a thing a if, and only if, some of the states of a in the presence of b differ from those of a in the absence of couplings with b. (Recall Vol. 3, Ch. 5.) Since space-time is in no state, we would not even know how to describe its interactions with things proper, for such interactions should cause state transitions. In conclusion, the absolutist views of space-time are methodologically as well as semantically untenable. Shorter: space and time do not exist by themselves.

The task of checking empirically the geometrical assumptions of general relativity is technically complicated and philosophically interesting. There are two methods: the direct and the indirect. The former consists in measuring some of the geometrical properties of a physical system such as a triangle composed by three electromagnetic signals. This was first done in 1964 using the Moon and two points on Earth, and its result confirmed the hypothesis that such a triangle is roughly spherical rather than plane. Similar measurements, using Mars, have corroborated that finding. However,

results such as these are global: they do not allow one to determine the components of the metric tensor. To do this one must assume general relativity itself – much as any test of mechanics uses some principles of mechanics. In fact what must be done to measure the g_{ab} is to solve a system of differential equations contained in general relativity, and plug into the solutions the values, found empirically, of certain parameters occurring in the solutions (Levi-Civita 1929.)

The indirect tests of general relativity are the most numerous of all and they consist in checking the "effects" predicted by the theory, i.e. some of its theorems describing the motion of bodies or the propagation of fields. A recent test (1979) was made with the help of a Viking spacecraft lander placed on Mars, which emitted an electromagnetic signal that grazed the sun. The signal reached our planet with an additional delay of 250 microseconds, which was explained as due to the decelerating effects of the solar gravitational field.

The interest of measurements of either of the two types for philosophy is double. Firstly, they show that the determination of the space-time metric is not done by transporting yardsticks – as Grünbaum (1973) and other philosophers believe – but by making time measurements. Secondly, they show that any detailed exploration of space-time is guided by general relativity rather than proceeding in a purely empirical fashion.

General relativity poses a great many additional methodological, foundational and philosophical problems. (See e.g. Bunge 1967c, Angel 1980, Torretti 1983, Friedmann 1983.) However, we must move on. We shall now leave the neat classical picture of the world as a system composed of entities – bodies and fields – endowed with properties every one of which has, at a given instant and relative to a given reference frame, a sharp or definite value.

4. Quantons

4.1. *Classons and Quantons*

We now enter the wondrous quantum world or, rather, the quantum-theoretical picture of the physical aspect or level of the world. This is still a world of material things in the large sense of 'material', which includes fields; and it is just as real, if experimentally more elusive, as the world studied by classical physics. However, the things to which quantum theory

refers are quite anomalous from a classical point of view. They are neither particles nor fields; they have neither definite trajectories nor precise shapes; their dynamical properties have seldom sharp values; once they clump into a system it may be hard to separate them; and their behavior, though lawful, is basically probabilistic.

We shall call *classons* the entities accounted for satisfactorily by classical physics (CP), *quantons* those that call for quantum physics (QP), and *semiquantons* (or *semiclassons*) those requiring theories that contain both classical and quantal concepts. All classons and semiquantons are ultimately composed of quantons. For example, the microscopic fibres composing this sheet of paper are classons composed of comparatively small molecules that can be described correctly only with the help of quantum theory. And the proteins and DNA molecules, which appear to be semiquantons, are likewise ultimately composed of quantons. However, "classon" is not the same as "macrophysical", and "quanton" does not coincide with "microphysical". In fact, some classons – such as dust particles – and semiquantons – such as biomolecules – are extremely small compared with perceptible bodies. And there are macrophysical quantons, such as black bodies, superconductors, superfluids, and neutron stars. (Moreover, if quantum cosmology is right, the entire universe before the big bang is likely to have been a single quanton.) Therefore our classon-semiquanton-quanton trichotomy is theoretically more profound and adequate than the usual micro-macro distinction, which is anthropocentric and therefore only practically useful.

There is no consensus about the relations between classical physics (CP) and quantum physics (QP). Indeed at least five different views have been discussed in the literature. A first view is that QP is merely an *extension* of CP, so that any physical problem, even the one posed by the collision of two trucks, could be treated quantum-theoretically if only one had enough patience and computer hours. (See e.g. Pauli *apud* Born 1971 p. 223.) This view is false because most of the problems in CP involve formulas containing concepts, such as those of shape, elasticity, and viscosity, that do not occur in the basic equations of QP. A second view is that CP is the *limit* of QP (e.g. for large quantum numbers or when Planck's constant is negligible). This is a half truth, for quantum mechanics includes parts of classical particle mechanics, and quantum electrodynamics parts of classical electrodynamics, on the average. However, QP has so far not been shown to contain hydrodynamics, the theory of elasticity, or thermodynamics. A third view is that CP is *included* in QP, which would result from enriching

CP with certain additional assumptions such as quantization rules. This influential view (Bohr 1949, Bohm 1951, Landau and Lifshitz 1958) is wrong because, in fact, QP contradicts parts of CP, and because QP entails parts of CP. (The motivation for this view is the operationist philosophy according to which every formula must be read in terms of possible experimental situations, which involve macrophysical systems, namely pieces of apparatus, that are allegedly describable in purely classical terms.) A fourth view is that QP and CP are *mutually incomparable* ("incommensurable"), so that neither reduces to the other even partially. Like the revolution operated by Galilei and Newton, which buried Aristotelian physics, QP would have buried CP. We have criticized this catastrophic view of scientific revolutions in Vol. 6, Ch. 13, Sect. 3.2. Suffice it to note here that (*a*) QP recaptures some classical results and (*b*) QP is preferred over CP in a number of research fields because, on comparing their respective results, those of QP have been found truer than those of CP. Our own or fifth view is that QP entails *some* CP in a modified manner (namely as average); that it is *very novel yet comparable* with CP; and that CP was *heuristically* instrumental in building QP, and is still indispensable to put QP to the *test*, but it is no part of the latter. (All of this can be proved rigorously only by axiomatizing quantum theory: Bunge 1967c.)

There can be no doubt that quantum theory is a good approximation to the truth – which is not to say that it is perfect. Thousands upon thousands of observations and experiments have confirmed its predictions in an amazing range of fields, from particle and atomic physics to solid state physics and astrophysics, usually with an astounding accuracy. Still, since its inception quantum theory has been the subject of vehement controversies that, far from abating as the theory became consolidated, have increased in recent years. (See Jammer 1974.) These controversies fall into two categories: *internal* and *external*. The internal controversies presuppose that quantum theory is substantially correct, and they bear mainly on the physical interpretation of its mathematical formalism. (The formalism itself is far from perfect in the case of quantum electrodynamics, noted for its trickeries, but this is a technical matter of no great philosophical interest.) The external controversies originate in a dissatisfaction with the theory as a whole, and they concern the possibility of replacing it with a classical or semiclassical theory. And the controversies of both kinds are largely motivated by philosophical reasons (or unreasons).

From a philosophical viewpoint most of the internal controversies revolve around the issue of realism, i.e. the problem of whether quantum theory

supplies a realistic representation of nature or, on the contrary, is centered on the knowing subject (the observer). The internal controversies are then substantially of an epistemological kind. And most of the external controversies revolve around the issue of hidden variables, or functions of a classical type, such as classical position and momentum, which at all times have sharp values instead of probability distributions. The external controversies are then substantially of an ontological kind. Together, the controversies of both kinds span a good deal of epistemology and ontology. There is also a marginal controversy over the kind of logic suitable for quantum theory, but we shall argue that this is a pseudoproblem.

Unfortunately the two main controversies, those over realism and determinism (or hidden variables), have often been mixed up – and this by scientists of the stature of Einstein and de Broglie, Bohm and d'Espagnat. Yet the two issues are quite different: whereas the problem of realism is epistemological, that of hidden variables is ontological. (See Bunge 1979c.) Therefore one can be a realist and either admit quantum theory or hope for a hidden variables theory. Or one can be a subjectivist while accepting or rejecting quantum theory. All four stands are consistent. As for the inconsistent positions, they are probably the most numerous and popular, here as in politics.

Since we hold that quantum theory, though perfectible like any other human creation, is substantially correct, we shall engage mainly in internal criticism. Our central target will be the standard or *Copenhagen interpretation* of the mathematical formalism of the theory. A major tenet of this interpretation is that it is meaningless to speak of quantons in themselves, i.e. separately from the knowing subject: that every formula of quantum theory ought to be read in terms of laboratory operations (Bohr 1934, 1935, 1936, 1949, 1958, Frank 1938, 1946, Heisenberg 1958, Pauli 1961, Rosenfeld 1953, 1961). Thus Bohr has been quoted as stating that "There is no quantum world. There is only an abstract quantum physical description. It is wrong to think that the task of physics is to find out how nature *is*. Physics concerns what we can say about nature" (Petersen 1963). If this were true, the scientific realism upheld in Vols. 5 and 6 of this *Treatise* would be false. But we shall argue that the operationist tenet is false, and shall do so by examining the basic ideas of quantum mechanics as well as some experiments relevant to it.

Now, criticism may be destructive or constructive. If the former, it may be interesting and suggestive, but it is unlikely to dislodge any received view. To be effective, criticism must be constructive: it must offer a viable

alternative. We shall therefore disregard the criticisms of the Copenhagen school made from a classical viewpoint, and shall have little patience with the attempts to "classicize" quantum theory by introducing "hidden" variables, i.e. functions that have definite ("sharp") values at all times. The nostalgic dream of a return to classical physics is just a dream, particularly after the experimental refutation of the entire family of hidden variables theories (Aspect *et al.* 1981, 1982), which will be examined in Sect. 6.2.

A constructive criticism of quantum theory can be either *reformist* or *revolutionary*: it may attempt to improve the theory or replace it with a radically new one, even farther removed from classical physics. We take it that a revolution is unnecessary at the moment, at least with respect to quantum mechanics, for there are no unavoidable inconsistencies or major experimental facts that escape the theory. (Quantum field theory is a different matter: here the problem of infinities is still to be solved in a rigorous manner.) So, reform seems to be the only reasonable course at this time. Now, reform may bear on the mathematical formalism (as is the case with every theory) or on the semantic assumptions ("correspondence rules") that confer a physical content upon the formalism. We shall deal here only with the latter, which we regard as the more urgent task and the reason that Nobeal laureate Richard Feynman has admitted that "no one understands quantum mechanics". We submit that a *realistic reinterpretation* of standard quantum theory solves its "mysteries" – without however restoring the pictorial quality of some parts of classical physics. So, ours will be a constructive reformist criticism of the Copenhagen school. (See also Bunge 1959b, 1967c, and 1973a.)

4.2. *The State Function and its Referents*

The very first question we should ask about any theory is: *What is it about*? The founders of quantum theory have proposed the most bizarre answers to this basic question. Thus Bohr (1958 p. 80 and *passim*) insisted that quantum theory refers only to "observations obtained under experimental conditions described by simple [classical] physical concepts". Heisenberg (1958 p. 52) stated that "the term 'happens' is restricted to the observation", and was of the opinion that quantum theory does not refer to nature but to our knowledge of the latter – whence it would be part of epistemology. And, in line with these opinions, Pauli (1961) claimed that the concept of a physical object independent of the observer is alien to quantum theory. It is not too surprising that such views were uttered and that they continue to be repeated in the literature: after all, quantum theory is still comparatively

young and it was invented at a time when operationism was the dominant philosophy of physics. What is remarkable, perhaps unique in the history of science, is that such views, worthy of Bishop Berkeley, were not expressed by contemporary philosophers but by outstanding physicists. What is even more remarkable is that they continue to be held in a dogmatic fashion, i.e. without any critical analysis. And what is downright scandalous is that cogent technical criticisms of such dogmas are rarely accepted by leading physics journals.

What can the philosopher do when faced with amazing opinions upheld by the greatest scientists of the day? If he is tender-minded he will adopt them to buttress some philosophical school. But if he is tough-minded he must subject them to critical examination, confronting them not with alternative dogmas – the way a schoolman would do – but with facts such as the activity of quantum theorists and experimentalists. That is, he must ask himself whether those opinions match the theory as well as the experiments in which the theory is involved. Let us do just this, starting with a search for the referents of quantum theory; matters of experiment will be deferred to Sects. 5.2 and 6.1. For the sake of simplicity we shall confine most of our discussion to non-relativistic quantum mechanics in its Schrödinger (or state space) formulation.

The search for the referents of a theory can be restricted to a semantic analysis of the basic or undefined concepts of the theory. Since such concepts are bound to occur in the axioms of the theory, we may focus our investigation on them. Here we shall examine the four most important postulates. (For an axiomatization of quantum mechanics see Bunge 1967c. For discussions on the interpretations of the mathematical formalism see Bunge, 1959b, 1967c, 1973a, 1983c.)

Let us start with the central postulate: *Schrödinger's equation*, which represents the evolution in time of the state function (or vector) ψ as determined by the total energy operator, or hamiltonian $\hat{H}$:

$$i\hbar\frac{\partial\psi}{\partial t} = \hat{H}\psi \tag{1}$$

where i is the imaginary unit, $\hbar$ Planck's constant, and t time. $\hat{H}$ is a function of the position and momentum coordinates of the quanton as well as of the external field, if any. In the absence of the latter, the hamiltonian reduces to $\hat{H} = \hat{p}^2/2m$ for a single "particle", and to

$$\hat{H} = \hat{p}_1^2/2m_1 + \hat{p}_2^2/2m_2 + H_{12} \tag{2}$$

for two entities, where $\hat{p}_1$ and $\hat{p}_2$ are the momentum operators, m_1 and m_2 the masses, and H_{12} the function or operator representing the interaction between the two quantons. The momentum operator is $\hat{p} = -i\hbar\nabla$, where ∇ is the gradient.

The state function (or vector) ψ is a complex valued function of the space and time coordinates. Its precise form is determined, up to constants, by the precise form of the hamiltonian $\hat{H}$ as well as by the initial and boundary conditions. In the simplest case, that of a single quanton free from external forces, ψ looks like a classical plane wave; in the case of an attractive central force (such as that exerted by an atomic nucleus on an electron), ψ looks like a classical spherical wave. These purely *formal* analogies gave rise to the misnomers 'wave function' and 'wave mechanics', just as the use of the hamiltonian formalism associated with the Schrödinger equation suggested the misnomer 'particles' for the ultimate specific referents of quantum mechanics. Bohr (1934, 1936, 1949) held that the undulatory and the corpuscular views were mutually complementary, whence we had to keep them both and play dialectical games with them. On the other hand Heisenberg (1930 p. 10) admitted that they are only "mental pictures" and "are both incomplete and have only the validity of analogies which are accurate only in limiting cases". Since they are indeed just analogies, and since they cannot be both correct, we shall adopt neither of them. We hold instead that the central referents of quantum theory are *sui generis* entities deserving a name of their own: *quantons*.

The independent variables and constants occurring in the Schrödinger equation for a single quanton, such as an electron in an electromagnetic field, are as follows. (Taken from Bunge 1977b.)

Symbol	*Concept*	*Referent*
x	Point in space	Space
t	Instant of time	Time
m	Quanton mass	Quanton
e	Quanton charge	Quanton
$\hat{p}$	Quanton momentum	Quanton
$\langle A_0, A \rangle$	Field four-potential	Field
$\hbar$	Planck's constant	-----

Since space and time are not physical things, they do not qualify as physical referents, or at least not on the same footing as the rest. (On a relational theory of space and time they are constructs referring ultimately to changing things: Vol. 3, Ch. 6.) The genuine physical referents of the

Schrödinger equation are then quantons and their environment. Of course the latter may consist of a measuring apparatus interacting with the quanton(s). But this is a special case; and quantum mechanics, being a fundamental theory, is not restricted to special cases. Besides, even if we specialize the hamiltonian operator to represent a quanton interacting with an apparatus, we still deal with a purely physical system. No amount of trickery can succeed in smuggling the concept of an observer into the above list.

A second axiom of quantum mechanics is the *eigenvalues equation* for an operator $\hat{A}$ representing an arbitrary dynamical variable ("observable") A. It reads

$$\hat{A}u_k = a_k u_k, \tag{3}$$

where a_k is the k-th eigenvalue (admissible value) of $\hat{A}$, u_k the corresponding eigenfunction (admissible solution), and k a real number. (We are not requiring that $\hat{A}$ be hermitian, so a_k need not be real. But we are assuming that $\hat{A}$ has a non-degenerate spectrum. This assumption is made for the sake of simplicity and it does not mutilate the philosophical problematics.) The simplest case is that of the linear momentum operator $\hat{p} = -i\hbar\nabla$. Its eigenfunctions are $\exp(ikx)$, where k is an ordered triple of reals; the corresponding eigenvalues are $p = \hbar k$. Another example is $\hat{H}u_k = E_k u_k$, which corresponds to a quanton in a stationary state – e.g. an atom in its ground state. In the case of the hydrogen atom the energy eigenvalues are $E_k = -g/n^2$, where n is a natural number and g the energy of the ground state of the system ($n = 1$).

According to the Copenhagen school, every a_k in (3) is one of the values that an *observer* may find when *measuring* the property A with a suitable instrument – of *any* kind. However, it is plain that (3) makes no allusion whatever to any observers, instruments, measurement techniques, or measurement operations. The only interpretation Equation (3) tolerates is a strict or literal one, namely that *a_k is one of the possible values of A* – whether or not we happen to measure it. So much for the semantic aspect of the question. The methodological aspect is just as clear: the results of a precision measurement depend not only on the thing measured but also on the measurement method, and they are rarely exact. (They can be accurate only if the eigenvalues are denumerable and widely separated.)

In general, a precision measurement of a property A will yield a whole set of values. One usually compresses this multiplicity into a formula such as

$$meas\ A = a'_k \pm \varepsilon_k, \tag{4}$$

where a'_k is the arithmetic mean of a set of measured values, and ε_k the corresponding relative error, which is characteristic of the measurement method as well as of k and of the size of that set. As a rule a'_k differs from the theoretical value a_k. If both were always identical, as implied by the Copenhagen interpretation, it would be possible to scrap all the research projects devoted to measuring the eigenvalues of all dynamical variables, since these eigenvalues would be given accurately and a priori, hence once and for all, by Equation (3). Fortunately, the workers at Cern, Dubna and Fermilab need not worry: the eigenvalues equation warrants only an objectivistic interpretation since, by hypothesis, the property A represented by the operator $\hat{A}$ is a property of an individual quanton, not of a microsystem including experimental equipment and experimenters.

We are now ready for our third postulate, the *superposition principle*, which we formulate in line with our objectivistic version of the first two principles. Let a be a quanton in state ψ_a, and let $\{u_k | k \in \mathbb{N}\}$ be the family of eigenfunctions of an arbitrary operator $\hat{A}$ representing a dynamical property A of a. Then every linear combination ψ_a of all the eigenstates of $\hat{A}$, i.e.

$$\psi_a = \sum_k c_k u_k,$$

where $|c_k|^2$ is the weight of the contribution of u_k to ψ_a, represents a possible state of a. Graphically: the c_k are the projections of ψ_a onto the axes u_k of the infinite-dimensional Hilbert space associated with $\hat{A}$.

(The state of a quanton of a given kind is representable as a point, or rather a ray, in a state space. The latter is an infinite-dimensional functional space – a Hilbert space. Just as we can coordinatize ordinary space in infinitely many ways, so every Hilbert space can be associated with a number of special grids. These grids are so many functional spaces, every

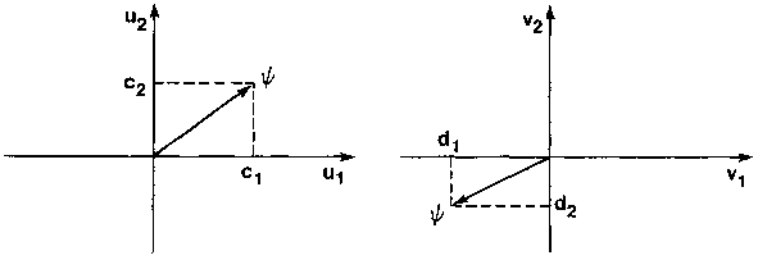


Fig. 2.5. Two different representations of the state ψ of a quanton: $\psi = \sum c_i u_i$ and $\psi = \sum d_j v_j$. The projections are the scalar products $c_i = (u_i, \psi)$ and $d_j = (v_j, \psi)$. Each auxiliary grid has infinitely many axes; in some cases this infinity is non-denumerable.

axis of which is a member of a definite family of functions, e.g. hypergeometric functions, Hermite polynomials, or plain trigonometric functions. And, just as a vector in ordinary space can be decomposed variously, according to the coordinate system that is chosen, so every direction in a Hilbert space can be represented variously in different functional spaces: see Figure 2.5. In other words, one and the same state vector can be projected onto the axes of as many auxiliary grids as desired. In particular, if the auxiliary grid is formed by the axes u_i, the state ψ will be decomposable in this way: $\psi = \sum c_i u_i$, where c_i is the projection of ψ onto the i-th axis u_i. By choosing a different auxiliary grid formed by the axes v_j, different projections are obtained: $\psi = \sum d_j v_j$. The choice of auxiliary grid is conventional, but not all auxiliary grids are mere mathematical fictions. In fact some of them correspond to dynamical variables. More precisely, every dynamical variable can be associated a peculiar functional space, the axes of which correspond to the precise, "sharp", or "well defined" values of the variable. Actually all the dynamical variables that can have simultaneous sharp values can be associated to one and the same auxiliary grid. In short, given quantons of a certain kind, quantum theory assigns them one basic state function together with a set of auxiliary functional spaces, or grids, every one of which is associated with one or more dynamical variables. These auxiliary grids are usually called 'representations'.)

The most popular interpretation of (5) is that it is a purely analytic tool allowing us to cope with our uncertainty concerning the state the quanton really is in. Actually, it is argued, the quanton is always in some definite eigenstate or other, so that its property A has a definite value – only we do not know which. According to this classicist interpretation, $|c_k|^2$ is the subjective probability that a be *found* to be in the eigenstate u_k. This is not our view: we agree with Bohr (1934) that the superposition principle, far from being a mere mathematical trick, has a deep physical meaning, and must therefore be taken seriously: whereas in CP it is often optional, in QP it is mandatory. But we interpret it in strictly physical (objective) terms, i.e. without reference to anyone's ignorance or uncertainty.

As is clear from our formulation, the superposition principle states that in general, i.e. except when the quanton happens to be in an eigenstate of some dynamical variable of it (i.e. when $\psi_a = u_k$), a quanton is in a linear combination of eigenstates. Therefore *in general physical properties lack sharp values*. Instead, each property A has an entire range of values, and each value a_k of A occurs in that range with weight $|c_k|^2$. In other words, generally $|c_k|$ is not sharply peaked. When it is sharply peaked – an exceptional case

– the situation corresponds to the classical case, in which the corresponding property has a sharp value.

Example 1 In general quantons are spatially diffuse. The so-called position indeterminacy or uncertainty Δx, which is a measure of the objective fuzziness in position, is rarely zero. (More in Sect. 5.1.) Since generally quantons are not at precise points in ordinary space, they do not have precise orbits either. Thus, according to quantum theory shape and bulk are not basic universal properties. This is why they play only a minor role in our ontology (Vol. 3, Ch. 6, Sect. 2.4). *Example 2* In general an electron is in neither of its two spin eigenstates $\frac{1}{2}\hbar$ (up) and $-\frac{1}{2}\hbar$ (down), but in a linear superposition of these states. (Classical counterpart: the precession movement of a spinning top in a gravitational field.) If the electron happens to go through an inhomogeneous magnetic field, either in nature or in the laboratory, its unpolarized state is projected onto either of the two spin states. More in Sect. 6.1. *Example 3* Classically, an unstable system is in either of two states: undecayed or decayed. Quantum-theoretically, as soon as such a system (e.g. a neutron) is formed, and as long as it has not disintegrated, it is in a superposition of the decayed states. More on this in a moment.

We take the superposition of eigenstates to be *typical of quantons* in contradistinction to classons. In other words, we regard quantons as being characteristically and objectively fuzzy. This point is crucial in order to understand one of Einstein's objections to quantum theory as well as the recent claim that experiment, by confirming the theory, has refuted scientific realism. Let us deal quickly with the former, leaving the latter for Sect. 6.2.

In their famous criticism of quantum mechanics, Einstein, Podolsky and Rosen (1935) took it for granted that every property of a physical object has a sharp value at any given instant, whether or not we know it. Bohr (1935, 1949) objected that it makes no physical sense to speak of things and their properties in themselves, i.e. apart from observation acts, for to be is to be measured. (For a criticism of this semi-Berkeleyan epistemology see Bunge, 1955a, 1959b.) In our view Einstein and his coworkers were right in asserting the realistic thesis that the world exists without our assistance. But they were wrong in supposing that the world is composed exclusively of things all the properties of which have sharp values (i.e. eigenvalues) at all times. This hypothesis is not *realist* but *classicist*: it amounts to claiming that the world is composed of classons, and therefore must be describable by classical (or neoclassical) theories.

We must distinguish realism from classicism. Our own view is *realist but*

not classicist. In fact we accept quantum mechanics but not its semi-subjectivist interpretations, in particular that of the Copenhagen school. We admit then that physical properties are of two kinds: definite or sharp, and indefinite or fuzzy. Thus, according to non-relativistic quantum mechanics (though not according to alternative theories), mass and charge are sharp in all circumstances. On the other hand position, momentum, angular momentum and energy are normally indefinite or fuzzy, in the sense that their values are normally ranges of numbers (or of n-tuples of numbers) rather than single numbers. This fuzziness has nothing to do with our ignorance or with the perturbations caused by some observation instruments. Observation merely confirms the *inherent fuzziness* of quantons. And experiment may reduce fuzziness in some respect (e.g. localization) while increasing it in another (e.g. velocity): see Sect. 5.1.

Schrödinger (1935a) thought he had refuted the superposition principle with an ingenious paradox. A closed box contains a cat and a cat-killing device activated by the disintegration of a radioactive atom. The latter has a decay probability of $\frac{1}{2}$ within the hour; hence the probability that the cat be dead after one hour is $\frac{1}{2}$. Schrödinger reasoned that, according to the Copenhagen school, as long as no one opened the box, the cat should be in a superposition of its living and dead states. This superposition would collapse on to either the living or the dead state, with equal probability, on opening the box: the observer creates the event.

Schrödinger claimed that such a state function cannot represent the objective states of affairs but only our imperfect knowledge of it since, whether we know it or not, the cat is either alive or dead. Ergo the corresponding probabilities are actually subjective not objective. And the superposition principle would be merely a mathematical trick without a real counterpart. (Moreover, all the properties would have sharp values at all times even while not being measured.)

The experimental physicist is likely to see no paradox, for he can install an automatically operating film camera inside the box and develop it afterwards to watch the course of events. Nor is the theoretical physicist likely to be persuaded by Schrödinger, for he may argue that the "live" and "dead" states are states of a *macro*system (the cat-in-the-box) that fails to satisfy the Schrödinger equation – so much so that the very concepts of life and death make no sense in quantum theory. (Even if one were to set up and solve the Schrödinger equation for that system, obtaining the solutions ψ_L and ψ_D, and the superposition

$$\psi = 2^{-1/2}(\psi_L + \psi_D), \tag{6}$$

one could reason as follows. At any given time, if ψ_L is non-zero, ψ_D vanishes, and conversely, so that the interference term $\psi_L^*\psi_D + \psi_L\psi_D^*$ vanishes at all times, which confirms that we have to do with a classical system not with a quanton. Another way of realizing that this is so is to sketch a subspace of the state space of the system. One then realizes that the L and D states are widely separated, so they cannot interfere with one another: see Figure 2.6. Furthermore, since the entropy of the dead cat is

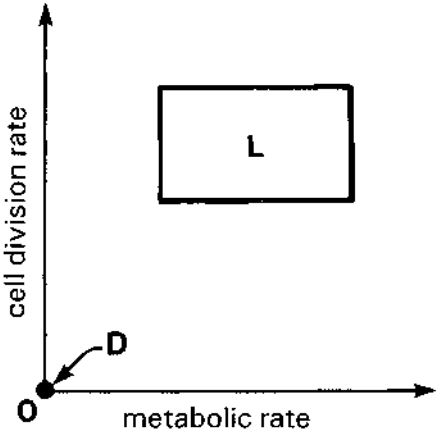


Fig. 2.6. Schrödinger's cat. Subspace of cat's state space formed by two typical biological functions. Point D (origin) represents dead state, region L represents all possible living states. D and L cannot interfere. Cats are classons not quantons: the superposition principles does not hold for them.

so much greater than that of the living cat, the state function of the dead cat – if it could be written out at all – would be a mixture rather than a pure state like (6).) In sum *there is no cat paradox*, but just an illegitimate application of the superposition principle.

So much for the first three postulates of quantum mechanics. The fourth will be examined in the next section.

5. CHANCE

5.1. *Probability*

So far we have interpreted ψ as the state function of a quanton, whether simple or complex. Knowledge of ψ summarizes our knowledge of the quanton concerned, as least so far as quantum theory is concerned. However, the representation of a state afforded by ψ is roundabout. Like lagrangians, hamiltonians and partition functions, ψ is a source function out of which particular properties can be constructed. One of these is the position probability density. That this is indeed so is what our fourth and last postulate states.

Born's postulate assigns a physical meaning to the state function ψ. It is a *semantic* postulate, not a law statement; what is a law statement is Schrödinger's equation (1) *together* with Born's axiom. We formulate Born's axiom as follows: "Let ψ_a be a solution of the Schrödinger equation for a quanton a. Then the probability that a be at time t in the region of space comprised between x and $x + \Delta x$ equals $|\psi_a(x, t)|^2 \cdot \Delta x$." The probability in question is a property of thing a in its environment, and therefore it is an objective probability. (For the objectivist concept of probability see Ch. 1, Sect. 4.2.) More precisely, it is the strength of the tendency or propensity that, when in state ψ_a, a be present in the volume element Δx situated at the tip of the vector x. Here again there is no suggestion of measurement: the principle is so general that it is stated for arbitrary quantons with whatever hamiltonian operators they may be assigned.

The Copenhagen school has its own version of the principle, namely: "$|\psi_a(a, t)|^2 \cdot \Delta x$ is the probability of *finding* a in Δx when the position of a is *measured* with the help of an *arbitrary* position-measurement device". This interpretation is illegitimate because ψ_a need not contain any measuring device coordinates – unless of course the corresponding hamiltonian $\hat{H}$ happens to contain them. But since the principle is utterly general, we should not care what $\hat{H}$ may look like, hence what the corresponding ψ_a may be. Moreover, any experimental physicist knows that the probability of finding a thing at a given place is not an intrinsic property of the thing: it depends not only on the probability of the thing being there, but also on the sensitivity of the search instrument and the skill of the experimenter. Thus, the probability of finding a needle in a haystack depends crucially on whether or not the explorer is wearing his eyeglasses and uses a metal detector. In summary, the operationist version of Born's semantic postulate is mathematically

illegitimate because it assumes coordinates that are not there; and it is methodologically false because it construes "observable" as a unary predicate.

According to the standard interpretation, ψ is "merely a subjective computational tool" (Kemble 1937 p. 328); moreover, ψ serves only to predict the possible outcomes of experiments done simultaneously or successively on similarly prepared quantons. On the other hand, in our realistic interpretation Born's axiom involves *objective* probabilities that may refer to *single* quantons and, moreover, to quantons not subjected to any experimental scrutiny. That the probabilities are objective is clear from the arguments of ψ, none of which refers to a knowing subject. (Remember from Sect. 4.2. that the Schrödinger equation, which ψ solves, contains no information about any observers.) To be sure the computed probabilities may turn out to be false, so that we may have to alter our premises in the light of new empirical information. But in this regard working in quantum theory is no different from working in classical physics. (See Newton 1980 and Ch. 1, Sect. 4.2.)

That the probability associated with the state function may refer to a single quanton, rather than to a statistical ensemble, is obvious for a similar reason: unless ψ does solve the Schrödinger equation for an ensemble of coexisting quantons, such as those composing a metallic body or a laser beam, *by hypothesis* it refers to a single (though possibly complex) quanton a, such as a molecule. This point is often misunderstood because the *test* of any calculated probabilities for single cases involves some ensemble of similar (coexisting or successive) entities. However, we must not confuse reference with test even though we must relate them. (See Vol. 1, Ch. 2, Sect. 2.6., and this volume, Ch. 1, Sect. 4.2.)

When a theoretical (e.g. probabilistic) statement is subjected to empirical tests, it must first be operationalized: i.e. it must be conjoined with indicator hypotheses and translated into laboratory or field jargon (Vol. 6, Ch. 11, Sect. 2.2 or 1973a, Ch. 10). Thus a quantum theoretical statement such as "The probability that individual thing a, of kind K, be in state s, is p", will have to be translated into something like this: "If an observation (or measurement) is performed on an ensemble (collection) of entities of kind K prepared in a similar fashion, a fraction near p of them should be found (if the theory is true) to be in state s".

If the difference between this statement, which refers to a collection under experimental control, and the previous statement, which refers to an individual, is overlooked, then one will mistakenly attribute quantum theory a

statistical character rather than a *probabilistic* one: i.e. he will infer that quantum theory is always about collections (in particular Gibbs ensembles) of similar entities, never about individual things. (This is in fact the statistical interpretation of quantum mechanics originally proposed by Slater (1929) and favored by Einstein (1948), Blokhinzev (1964), Ballentine (1970), de la Peña (1979), and a few others.) As we saw above, this opinion on the referents of quantum theory contradicts the fact that in many cases ψ is made to refer, *by hypothesis*, to a single quanton in a given environment – e.g. an electron in an electromagnetic field, or even in free space. It also contradicts the very existence of quantum statistical mechanics, which does in fact refer exclusively to ensembles of coexisting quantons. And it is *experimentally refuted* by electron diffraction experiments, which can be done with a single electron at at time (Sect. 5.2). For all these reasons we should speak of the *probabilistic*, not the *statistical*, interpretation of quantum theory mediated by Born's axiom.

(Some people distrust the very notion of the probability of a thing being in a given state. In some cases the objection stems from the classical principle that every thing in in some sharp state, i.e. an eigenstate, at all times, so that the very notion of a linear combination of states makes no sense to them. At other times the objection originates in the operationist thesis that we can measure only frequencies of changes of state, such as the emission or absorption of a photon by an atom. The unease should disappear upon reflecting that (*a*) quantum theory calculates the probabilities of states, not only of events, and (*b*) the probability of a thing being in a given state can be construed, and sometimes even calculated, as the sum of the probabilities of all the possible processes, or paths, leading to that state, i.e. as the sum of all the transition probabilities.)

Our objectivist version of Born's principle clarifies the following point on position coordinates. Classical mechanics contains a position coordinate X that assigns to each particle, relative to each reference frame, at each instant, and on each distance unit, a triple of real numbers, namely the components of the position vector along three coordinate axes. (I.e. X: $P \times F \times T \times U_d \to \mathbb{R}^3$. Similarly for points in a field.) Quantum mechanics contains no such function. The position operator $\hat{x}$ is not a vector the components of which have definite values; only its probability density $|\psi|^2\hat{x}$ has sharp values at all times. Hence quantum theory *cannot assign any precise positions* to quantons. (Mathematically: $\hat{x}$ has no well-behaved eigenfunctions, hence no well defined eigenvalues.) And, since quantons have in general no sharp positions, they have in general *no sharp trajectories*

either. (To be sure one can compute $d\hat{x}/dt$ to obtain $[\hat{p} - (e/c)A]/m$. However, this entails only that the eigenvalues and the mean values of the velocity are those of the corresponding kinetic momentum divided by the mass. We cannot integrate the preceding equation of motion to obtain the quanton trajectory. Again, since there is no sharp position there is no sharp orbit.) In brief, quantons are not point-like. Nor are they wave-like. (This can be seen e.g. by comparing the Galilei transform of a classical wave with that of a ψ "wave". Whereas the former remains unchanged, the latter does not. Indeed, the transformed ψ "wave" contains terms representing the momentum and kinetic energy of the moving frame.)

The probability density $|\psi(x, t)|^2$ allows one to calculate all the statistical parameters defined in probability theory, in particular averages, momenta, and mean standard deviations. In particular we can compute the mean standard deviation of the distribution of values of any dynamical variable, such as the position and the momentum of a quanton in a given state. (The average of a dynamical variable A is $\langle \hat{A} \rangle = (\psi, \hat{A}\psi)$, the nth momentum of A is $\langle \hat{A}^n \rangle = (\psi, \hat{A}^n\psi)$, and the mean standard deviation of $\hat{A}$ is given by $(\Delta\hat{A})^2 = \langle \hat{A}^2 \rangle - \langle \hat{A} \rangle^2$.) Moreover, if we settle for inequalities we can obtain certain results of the utmost generality, i.e. without making any precise assumptions about the nature of the quanton(s) concerned, except that they "obey" quantum theory. Let us do this in the case of the most celebrated and misinterpreted of all the formulas of quantum theory.

Heisenberg's inequalities relate the mean standard deviation $\Delta\hat{x}$ of the position, and $\Delta\hat{p}$ of the momentum, of an arbitrary quanton in an arbitrary state at any given time. They read:

$$\Delta\hat{x} \cdot \Delta\hat{p} \geq \hbar/2 \tag{7}$$

for every component. The preceding formula is a rigorous consequence of only three general premises: (*a*) the commutation equation $\ulcorner \hat{p}\hat{x} - \hat{x}\hat{p} = -i\hbar \urcorner$, obviously satisfied by $\hat{p} = -i\hbar\nabla$; (*b*) the definition, recalled a moment ago, of the mean standard deviation of an arbitrary dynamical variable, and (*c*) a certain mathematical inequality (Schwartz's) devoid of physical meaning.

To derive (7) we need not know anything about the quanton except that it "obeys" quantum mechanics. (We do need a precise knowledge of the quanton and its environment if we wish to obtain an equality.) In particular, no measurement apparatus, much less an observer, is referred to in the premises leading to (7). The observer and the apparatus occur only in popular heuristic presentations as well as in their philosophical discussions.

All (7) "says" is that *the width $\Delta\hat{x}$ of the position distribution is inversely proportional to the width $\Delta\hat{p}$ of the momentum distribution*, and this regardless of the type of quanton and the measurement procedure: see Figure 2.7. Whatever else one may wish to read into (7) is sheer fabrication: it has not a mathematical leg to stand on and can only be justified by philosophical bias.

In the Copenhagen camp there is no consensus on the way (7) is to be interpreted – an indicator of the uncertainty that characterizes that interpretation. The most popular interpretations of (7) are the following five:

(i) The formula represents *mutually complementary measurement devices*: anything that measures x with any precision destroys the very possibility of an exact measurement of p and conversely. *False*, because the premises that

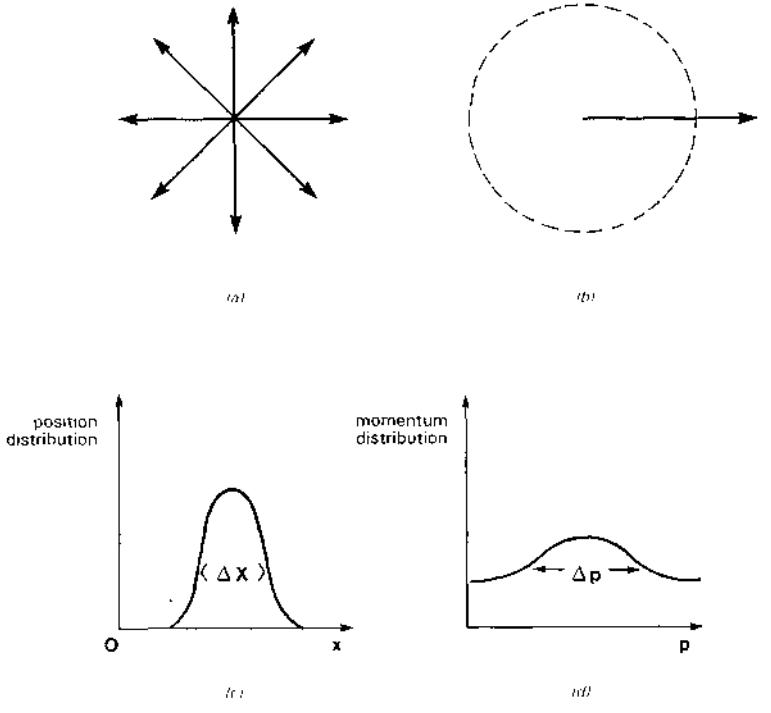


Fig. 2.7. Heisenberg's ("indeterminacy", "uncertainty") inequalities ("relations"). (*a*) A fairly well localized quanton moves at all speeds in all directions. (*b*) A quanton moving at a definite speed is poorly localized (unless constrained by walls or fields). (*c*), (*d*) The smaller the position spread, the greater the momentum spread.

entail (7) make no reference whatsoever to any measurements: they hold for all quantons in any states, whether free or acted upon by measuring instruments.

(ii) The mean standard deviations related by (7) are *caused* by the measurement act, for which the smaller the object the greater the disturbance. *False* for the above reason and also because it presupposes the classical view that the properties of all physical things have sharp values at all times.

(iii) The deviations occurring in (7) are plain measurement errors. *False* for the two reasons given when examining (ii).

(iv) The dispersions involved in (7) are *widths of the wave packet* (in ordinary space and in momentum space respectively). *False* because ψ does not represent a wave. (Electrical engineers are familiar with an analogous inequality that holds for classical waves, but this is another story.)

(v) The standard deviations are *uncertainties* in the physicist's mind concerning the exact position and momentum of the quanton. *False* because (7) refers to quantons, not to human brains, and because the assumption that quantons have precise positions and momenta at all times (only we do not know them exactly) is alien to quantum theory. Uncertainty is a feature of the Copenhagen school, not of quantons.

In short, all five popular interpretations of Heisenberg's theorem are unwarranted by the premises that entail it. (Moral: Thou shalt not talk in the theorems whereof you kept silent in the axioms and definitions. More bluntly: Don't smuggle.) Heisenberg's inequalities represent an objective non-classical property of quantons that has nothing to do with measurements or mental states. Being essentially probabilistic, they refute classical (Laplacian) determinism in both its ontological and its epistemological versions. (According to the former the future is uniquely *determined* by the present; according to the latter the future can be *predicted* exactly from an accurate knowledge of the present.) However, (7) does not justify radical indeterminism or the denial of lawfulness, because (7) happens to be a law statement. (See Bunge 1959a for general determinism.)

Heisenberg's indefiniteness inequalities show, among other things, that quantons are not point particles. (Another argument is that the eigenfunction of the position operator is a delta, which has an infinite norm and is therefore to be excluded from the formalism.) Not being point particles, quantons have no precise trajectories. But they do have average trajectories that coincide with the classical ones. (Roughly, a classical orbit is the trajectory of the center of the probability distribution.)

The Heisenberg inequality (7) is a special case of a general formula. Let $\hat{A}$ and $\hat{B}$ be dynamical variables (in either quantum mechanics or quantum field theory) satisfying a commutation equation of the form

$$\hat{A}\hat{B} - \hat{B}\hat{A} = i\hat{C} \tag{8}$$

where $\hat{C}$ is a hermitian operator, i.e. one with real eigenvalues. (Examples: the components of the orbital angular momentum and of the spin.) By a reasoning similar to the one resulting in (6) one obtains

$$\Delta\hat{A} \cdot \Delta\hat{B} \geq \tfrac{1}{2}|\langle \hat{C} \rangle| \tag{9}$$

where $\langle \hat{C} \rangle$ is the (spatial) average of $\hat{C}$. Pairs of dynamical variables that satisfy equations of the form (8) are said to be *conjugate* to one another. They are also said to be *mutually complementary*, in the sense that, the sharper or more peaked the distribution of one of them is, the more widely distributed its conjugate is.

The usual operationist or Copenhagen interpretation of the commutation equation (8) and its consequence (9) is this: "If two 'observables' (dynamical variables) are not simultaneously measurable, then their corresponding operators do not commute, and conversely. In other words, non-commutativity amounts to lack of simultaneous measurability". We reject this interpretation because, as noted above, no reference to measurement is made in either (8) or (9). Our own interpretation of Heisenberg's generalized commutation equation (8) and inequality (9) is this: "If two dynamical properties of a quanton fail to *have* sharp or definite values at the same time, then their corresponding operators do not commute. (Put it negatively: a quanton is never in the eigenstates of non-commuting dynamical variables.) And, not *having* sharp simultaneous values, no measurement can *find* them: we cannot measure something that is not there".

Our realist interpretation of (8) and (9) turns the Copenhagen interpretation upside down: *it is not that measurement precludes our finding simultaneous sharp values, but that the non-existence of the latter prevents us from measuring them*. Our interpretation is also at variance with classical realism (of the Einstein-de Broglie-Bohm type), according to which quantons do have sharp properties (represented by hidden variables) at all times, only quantum theory is incomplete, so a new theory, including hidden variables, is needed. In our view this is only wishful thinking and a hang-up from classical physics. (We shall return to it in Sect. 6.2.) The basic constituents of the universe are somewhat fuzzy, yet just as real as the neat macrosystems they constitute.

The Heisenberg inequalities (7) constitute the most popular example of the general formula (9). Another important though less well known case occurs in quantum field theory: it involves the operator $\hat{N}$ representing the number of field quanta (e.g. photons) and the operator Θ representing the corresponding field phase. These two are conjugate dynamical variables: they are related by $\hat{\Theta}\hat{N} - \hat{N}\hat{\Theta} = i$. (More precisely, $\hat{\Theta}_{k\lambda} \cdot \hat{N}_{k'\lambda'} - \hat{N}_{k'\lambda'} \cdot \Theta_{k\lambda} = i\delta_{kk'} \cdot \delta_{\lambda\lambda'}$, where k and λ are the wave and the polarization vectors respectively.) The corresponding standard dispersions inequality is, in accordance with Equation (9),

$$\Delta\hat{\Theta} \cdot \Delta\hat{N} \geq \tfrac{1}{2}. \tag{10}$$

In words: the scatters in the particle number and in the wave phase are mutually dual or complementary. The ideal limit cases are

$\Delta\hat{N} \to 0, \quad \Delta\hat{\Theta} \to \infty$	a sharp or well defined particle number but no definite wave;
$\Delta\hat{\Theta} \to 0, \quad \Delta\hat{N} \to \infty$	a sharp or well defined wave but no definite particle number.

These two are only ideal cases: actually quantons behave neither as particles nor as waves. If preferred, every quanton is in a superposition of particle and wave states.

We take Equation (10) to be a precise formulation of the *complementarity principle* (actually a theorem), stating that the particle and wave *pictures* of matter are mutually complementary (Bohr, 1934, 1936, 1949). Actually Bohr did not state the "principle" in relation to (10) but to (7), and he did not say that it shows the dual nature of *matter* but rather that it exhibits the complementarity between different types of *experimental set-ups* (Bohr 1949). However, (7) does not contain any undulatory property. And (10) cannot be interpreted in terms of measurements, if only because phases are not measurable: only phase differences are. (Interestingly, the phase difference $\hat{\Theta}_1 - \hat{\Theta}_2$ between the two quantons and their total particle number $\hat{N}_1 + \hat{N}_2$ commute. Hence we can measure accurately and at the same time their phase difference and the total particle number, but we cannot know which "particles" go with which "waves".)

Bohr's complementarity "principle" (10) has been endlessly discussed in the literature for being regarded as the clue to the interpretation of quantum theory. However, there is no consensus on either its formulation or its significance. Indeed the following alternative versions of it have been defended: (*a*) it describes the dual, or even contradictory, nature of *matter*,

i.e. the fact that on the quantum level things are both corpuscular and undulatory; (*b*) it expresses the complementarity of *measurement set-ups*, i.e. the fact that, whereas certain devices measure corpuscular properties, others measure undulatory ones; (*c*) it expresses the duality and complementarity of the particle and wave *pictures* of nature. (That the three alternative views are different and are usually conflated is hardly noticed.)

In our view each of the three versions of the complementarity "principle" contains a grain of truth and none is central to quantum theory because, as shown by (10), the particle and wave "aspects" (of quantons, measurement devices, and descriptions) are only *ideal limits* and moreover *mutually exclusive ones*. (This destroys, in particular, the thesis that complementarity illustrates dialectics.) We have come to understand that quantons are much too rich to be describable in classical terms such as "particle" and "wave". We have learned that the referents of quantum theory are neither corpuscles nor fields, although they do possess, in certain *special* cases, particle-*like* properties, and in others field-*like* properties – e.g. photons transport quanta of energy, and electrons can diffract. So, although we retain the complementarity "principle", we reinterpret it in objective terms and assign it a modest role.

The following analogy, (Lévy-Leblond and Balibar 1984 p. 67), illuminates the situation. A cylinder presents both circular and rectangular features, but it is neither a circle nor a rectangle. And saying that the circle and the reactangle are mutually complementary will not help understand what a cylinder is – whereas exhibiting a solid cylinder or the corresponding mathematical formula will help. Yet it is true that, in exceptional circumstances – namely when seen along its axis – a cylinder looks *as if* it were a circle; and in others – namely when looked at from the side – it *appears* as a rectangle. Likewise a quanton appears as a particle when it collides with (actually is scattered by) another quanton; however, the collision cross section is anything but the classical (geometrical) one, for it depends upon the wavelength of the quantons. And in other circumstances, e.g. when diffracted by a crystal lattice, a quanton *appears* as a field; however, a calculation of the diffraction pattern involves knowledge of its mass and spin if any. The truth about quantons is told by quantum theory (in its realistic interpretation), not by partial classical analogies that were inevitable, and even had some heuristic value, in the early confused days of the theory.

Finally we must say a word about the popular view that the Heisenberg inequalities and their kin set a *limit on our knowledge* of nature by showing, e.g., that we cannot know at the same time the exact position and the exact

momentum of an electron. Obviously, this interpretation presupposes that the electron *has* an exact position and an exact momentum at all times. If this were true then our knowledge would indeed be deficient and incapable of being improved on. However, that is not what quantum theory states: as we saw a while ago, quantons have distributions of position and momentum (and angular momentum and energy, etc.): sharp values are exceptional and moreover ideal cases. Hence *there is no sharp value to be known*: we cannot undertake to find out what is not there. In other words, the popular view we are discussing presupposes that the referents of quantum theory are classons, not quantons. And it overlooks the fact that Heisenberg's inequalities and their kin do not refer to our knowledge but are (derivative) law statements referring to physical objects whether or not they are under observation. In sum, Heisenberg's inequalities and their kin, far from setting limits upon human knowledge, constitute an important addition to it.

5.2. *Double Slit and Double Logic*

Let us now investigate a puzzling problem that is usually treated in an either a classical or a subjectivistic manner, and that has occasionally suggested the need for a change in logic: the *double slit experiment.* (See Figures 2.8

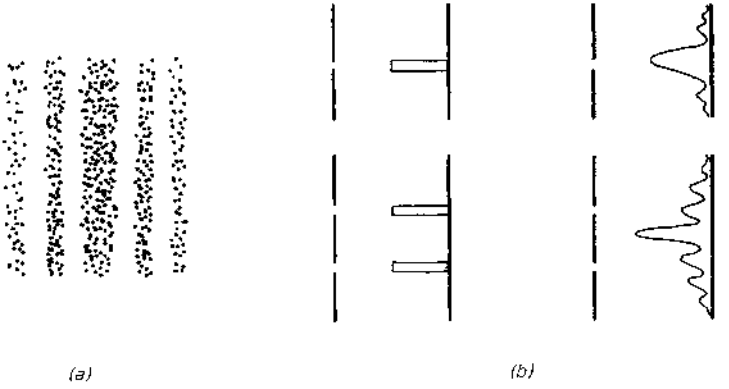


Fig. 2.8. "Interference" patterns. Quantons are emitted by a source, pass through a slit system, and strike a detecting screen, e.g. a photographic plate. (*a*) The "interference" pattern of individual impacts of quantons passing through a double slit system. (*b*) Comparison of behavior of classical corpuscles and classical waves. See Jönsson (1974) for electron diffraction photographs from a single slit and multiple slits. Caution: the intensity curves occurring in that paper were calculated classically using Huyghens's principle.

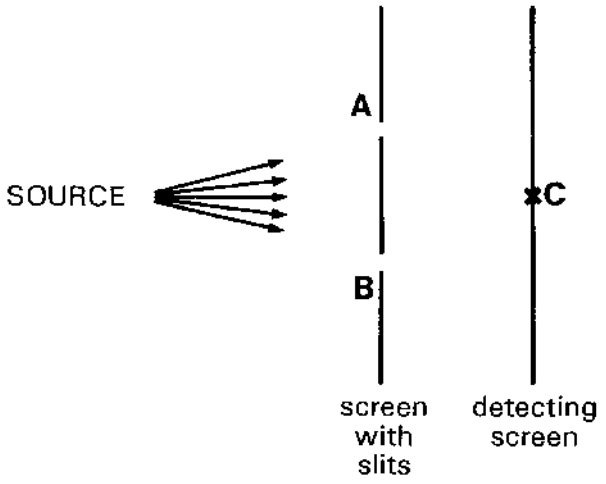


Fig 2.9. The double slit experiment as alleged empirical refutation of distributive logics and hence empirical support for quantum logics. Source of fallacy: the dogma that quantons are particles.

and 2.9.) If quantons (e.g. electrons) are emitted one at a time (e.g. by an electron "gun"), those that make it through the slit system strike the recording screen producing a flash (fluorescent screen) or a black dot (photographic plate). The distribution of impacts on the recording screen is not the superposition of the distribution resulting from the single slits (i.e. when one of them is closed). A qualitatively new pattern emerges, that is explained in terms of the interference of real waves in the case of light, and the interference of ψ "waves" in the case of electrons and other massive quantons. These distributions are statistical or mass outcomes: the individual thing, be it photon or electron, is recorded as a small (though extended) spot. As the intensity of the incoming beam increases, the individual spots arrange themselves into bands that increasingly resemble the interference bands produced with high intensity light sources. That is, the wave aspect of matter (whether massive or not) is an *emergent property possessed by aggregates of quantons* and absent from the individual quantons. The individual quantons behave neither as classical particles nor as classical fields. (Recall Sects. 4.1 and 5.1.)

The double slit experiment has been interpreted in a subjectivistic way, as proving that it is up to the experimenter to "conjure up" now the particle

aspect, now the wave aspect, by varying at will the source intensity or the slit apertures. It is certainly true that the experimenter has this power – but so has Mother Nature, which owns plenty of crystals and natural gratings that do the same tricks without deliberation or research grants. The particle and wave aspects are objective properties of matter. But they are only the two ends of a rich gamut of behavior that cannot be compressed into the classical framework. (Recall Sect. 5.1., in particular formula (10), showing that the more pronounced the corpuscular aspect, the less enhanced the wave aspect and conversely.)

Through which slit does the quanton go? According to the strict Copenhagen interpretation, this question is meaningless unless one makes a position measurement – e.g. closes one of the slits, thus making sure that the quanton has passed through the other. (See e.g. Schiff 1947, Heisenberg 1958.) But of course if we do this the question evaporates; so, it cannot be answered at all. The answer given by other authors is different, namely this: When both slits are open, the quanton goes through both of them in such a manner that there is interference between the two waves emerging from them. (Each quanton is said to interfere only with itself: Dirac 1958 p. 9) We agree that the quanton goes through *both* slits and that, for strong beam intensities or long exposure times, the process can be *mimicked* by classical waves. (In fact the interference patterns are usually calculated using Huyghen's principle for light waves, not by solving the Schrödinger equation, which has no closed form solution in this case.) However, we do not admit that the process is one of classical diffraction, for the state function ψ does not represent a classical wave.

We submit that the correct answer is obtained using the superposition principle: when the two slits are open the state of the incoming quanton is a linear combination of the states corresponding to the two slits. To put it in symbols suggested by Figure 2.9:

$$\psi = a\psi_A + b\psi_B, \quad \text{with} \quad a, b \in \mathbb{C}. \tag{11}$$

The resulting probability density is then

$$|\psi|^2 = |a\psi_A|^2 + |b\psi_B|^2 + a^* b\, \psi_A^* \psi_B + ab^*\, \psi_A \psi_B^*. \tag{12}$$

The first two terms are the quantum correlates of the corresponding classical ones, and the last two are the so-called interference terms. However, this is only a mathematical analysis: quantum theory does not authorize us to interpret the first two terms in a corpuscular manner and the last two in an undulatory one. The physical state is not split. The two components of ψ

in (11) are *inseparable* even though they are distinguishable. (More on inseparability in Sect. 6.2.)

Finally, let us examine what happens if one assumes that quantons are particles, so that every quanton goes either through one of the slits or through the other but never through both. In this case one is tempted to reason directly on probabilities rather than probability amplitudes (state vectors). One starts by identifying the three events of interest as suggested by Figure 2.9:

A = ⌜The quanton passes through slit A⌝.

B = ⌜The quanton passes through slit B⌝.

C = ⌜The quanton arrives at some point C on the recording screen⌝.

The probability of the quanton arriving at C on the recording screen is then

$$P(C) = P(C \,\&\, (A \vee B)). \tag{13}$$

If one assumes, in line with ordinary logic, that conjunction distributes over disjunction, one can expand the RHS of the previous equation, obtaining

$$P(C) = P(C \,\&\, A \vee C \,\&\, B). \tag{14}$$

Now, since the quanton is assumed to be a particle, it will go through either slit A or slit B but not through both. In other words the possibilities $C \,\&\, A$ and $C \,\&\, B$ are *mutually exclusive*, so that (14) becomes

$$P(C) = P(C \,\&\, A) + P(C \,\&\, B). \tag{15}$$

However, this formula is false, because it does not contain the symbols representing interference: recall Equation (12). Putnam (1976), Friedman and Putnam (1978) and others conclude that the root of the trouble is the hypothesis of *distributivity* employed in deriving (14) from (13). But physicists never bother with logic; moreover they employ classical mathematics, which has ordinary (distributive) logic built into it. On the other hand physicists calculate probabilities indirectly, namely from state vectors, not directly. And they do not assume that the possibilities $C \,\&\, A$ and $C \,\&\, B$ are mutually exclusive, so they do not obtain the false formula (15). In short, the claim that the double slit experiment forces one to give up distributivity, and hence to adopt a quantum logic, presupposes that quantons are classical particles. (See Selleri and Tarozzi 1978.) We thus confirm a conclusion arrived at in Ch. 1, Sect. 3.2 through a different route: *the logic underlying quantum mechanics is classical*. Therefore quantum logics are idle. (The same conclusion is obtained by axiomatizing quantum mechanics and showing that it employs only classical mathematics. See Bunge 1967c.) What are nonclassical are the quantons, and this is why we need quantum physics rather than classical physics to account for them.

The upshot of this section is as follows.

(i) Quantum physics accounts for real entities – quantons – unknown to classical physics;

(ii) for all its novelties,quantum physics dovetails partially with classical physics;

(iii) quantum theory is substantially correct, but the standard (Copenhagen) interpretation of its mathematical formalism, which is semi-subjectivist, is false and must be replaced with a realist interpretation;

(iv) quantum theory refers to single quantons or systems of such, the states of which it describes by state functions (vectors) that are not directly observable;

(v) in quantum theory probabilities cannot be assumed or calculated from other probabilities but must be calculated ultimately from state functions; the quantum-theoretical probabilities are objective;

(vi) the most general state function of a quanton is a superposition of states corresponding to sharp values of some property of the quanton, such as its energy; sharp values are the exception;

(vii) the state function extends over a region of space that depends on the nature of the environment of the quanton; e.g. an electron in a box occupies the entire box (though it is not homogeneously distributed in it), and an electron diffracted by a two slit screen passes through both slits;

(viii) the state of a quanton is extremely sensitive to environmental variations, whether natural or artificial, which can now enhance the "corpuscular aspect", now the "undulatory aspect" of the quanton;

(ix) the observer is not among the referents of quantum theory; and the apparatus occurs only when it is explicitly represented in the hamiltonian, hence also in the state function, of the system;

(x) the logic underlying quantum theory, whether in its Copenhagen or in its realistic interpretation, is classical logic: quantum logics are only games.

The following section, dealing with two other unclassical ("paradoxical") features of quantons, will reinforce the above conclusions.

6. Realism and Classicism

6.1. *Measurement and State Projection*

The main epistemological problem about quantum theory is whether it represents real (autonomously existing) things, and therefore whether it is

compatible with epistemological realism. (The latter is the family of epistemologies which assume that (*a*) the world exists independently of the knowing subject, and (*b*) the task of science is to produce maximally true conceptual models of reality. See Vol. 6, Ch. 15, Sect. 2.2, and note that the question of reality has nothing to do with scientific problems such as whether all properties have sharp values and whether all behavior is causal.)

Except for a few unfortunate madmen, people – even quantum theorists and philosophers – go about their daily business taking epistemological realism for granted: consistent non-realists, when found, are locked up and treated. In particular, experimentalists handle pieces of apparatus which they use to observe or transform certain physical objects, without ever doubting that at least those bits of laboratory equipment are real – and with them their microphysical components. (See Born, 1953.) And theorists keep trying to improve the adequacy of our ideas by modifying their theories, rather than engaging in incantations aimed at subduing reality to their preconceived ideas. The very notions of objective test and objective truth presuppose the basic principles of epistemological realism.

Yet one often reads in authoritative books and papers that quantum theory has refuted epistemological realism, at least with regard to microphysical entities. (Those texts never question the reality of the apparatus or the observer, although they admit tacitly that their ultimate components have no autonomous existence.) The main pillar of the non-realist interpretations of quantum theory is a certain view on measurement and on the projection (reduction) of the state function that is involved in measurement. (See Wheeler and Zurek Eds. 1983 for the standard view and some criticisms of it.) We shall show that this view is incompatible with both the theory and the practice of measurement.

Although measurement has become commonplace not only in science and technology but also in indutry and trade, and even in the modern home, some theoretical physicists and philosophers hold quaint views about it. (See Sect. 1.1 of this chapter, and Vol. 6, Ch. 11, Sect. 3.1, for general treatments of measurement.) However, if pressed any experimental physicist will admit that every measurement serves either *to find out or to alter* one or more properties objectively possessed by some real or at least putatively real object. Moreover, every experimentalist will inform us that a microphysical measurement, i.e. one performed on a microphysical entity, be it quanton, semiquanton, or classon, is a process in which the states of the microthing become *correlated* with those of the measuring apparatus, so that observing the latter allows one to infer the former. In other words, the measurement

process involves an *amplification* or micro-macro "translation"; moreover, the process is *often irreversible*.

Amplification is the price exacted by our comparatively poor sensory apparatus, and irreversibility the price paid for recording. If we were the size of molecules we might not need amplification. And whenever we let our senses do the recording, they engage in some irreversible process such as a photochemical reaction in the retina. The counting of quantons is a familiar example of microphysical measurement. Thus when an alpha-particle hits a ZnS crystal, it causes the emission of a bunch of photons capable of stimulating the human retina in the dark; and when a charged particle goes through a Geiger-Müller counter, it triggers an electron avalanche causing a click, which in turn can stimulate the human cochlea.

The term 'measurement' is sometimes used to denote the *preparation* of an entity – e.g. a steam jet or a molecular beam – that precedes the measurement proper. (For the difference between measurement and preparation see e.g. Margenau 1978 Ch. 12) The preparation may involve some measurements but these are auxiliary and not always required. For example, the production of a steam jet need involve no measurement: a tea kettle suffices.

At other times 'measurement' is misused to denote *any interaction* of an entity with its environment. Thus it has been said that, in the case of the radioactive nucleus shielded from its macrophysical environment, "the experimental apparatus can be identified with the electrons surrounding the nucleus" (Fonda *et al.* 1978 p. 623). It has also been said that the collision of a proton with other particles in an atomic nucleus "essentially constitutes a measurement" (Horwitz and Katznelson, 1983, p. 1184). These are obvious mistakes, for neither the atomic electrons nor the nuclear protons yield measurement results. The mistakes originate in the Copenhagen article of faith that it is senseless to talk about things (e.g. atomic nuclei) in themselves, i.e. apart from any measurement apparatus. And the mistake involves a logical fallacy: from the truth that every measurement involves an interaction it does not follow that every interaction is a measurement.

However, the worst misconception of measurement is its identification with the subjective experience of *taking cognizance* of the outcome of measurement. This overlooks the fact that an essential point of measurement is to find out how things are independently of our perceptual and conceptual errors. (Thus, if we need to know a distance with some accuracy we measure it instead of just estimating it by eye. And if we wish to know whether a certain flying object is a flying saucer or a plasmoid, we attempt

to take some measurements of it.) Besides, the subjectivist misconception ignores that measurements can be fully automated, and their outcomes stored in computer memories or printouts that may be consigned to oblivion. Of course in such case the measurements will be useless, but the point is that they are measurements and physical processes not mental ones. This is why they are accounted for by strictly physical theories. (Only some measurement errors call for psychological considerations.) Whether or not we look at the apparatus once the measurement results are in is irrelevant to the outcome of the measurement process. This is what laboratory experience teaches us and what every realistic theory of measurement states. After all, "Quantum mechanics may have some rather bizarre and peculiar features, but it is not a subjective and mystical creed" (Gottfried 1966 p. 183).

So much for some popular misunderstandings. Let us now note an important distinction seldom made in the quantum theoretical literature on measurement although it was drawn long ago by Pauli (1958 pp. 72–73). This is the distinction between measurements of the first kind, or non-disturbing, or *unobtrusive*, and measurements of the second kind, or disturbing, or *obtrusive*. It is usually stated that all microphysical measurements are obtrusive. But this is not so: thus, measuring the wavelength of a light wave is an unobtrusive operation because it does not alter the value of the property being measured. On the other hand measuring the polarization of a light wave by means of a nicol prism is an obtrusive act: the apparatus does not merely render manifest the state of polarization but produces it. A more familiar example of an obtrusive measurement is that of taking the temperature of a small liquid body by inserting a thermometer in it: this procedure may alter the original liquid temperature because of the heat exchange between the two things.

Ideally, the repetition of an unobtrusive measurement can yield the same result – provided sufficient time has elapsed for the recovery of the measuring instrument. On the other hand the repetition of an obtrusive measurement is bound to yield a somewhat different result. In the case of classons the disturbance introduced by the measuring device can be corrected for by supplying the apparatus with a compensation device; and in principle it can always be calculated theoretically and thus be corrected conceptually. This is not generally true of quantons: in this case the disturbance may neither be compensated for nor calculated.

A much-discussed, though perhaps still not well-understood, example of a microphysical measurement is that of the spin of a massive quanton, such

as an ion, with the help of a Stern-Gerlach apparatus. A beam of quantons enters an inhomogeneous magnetic field that splits the beam into two roughly equal parts that impinge upon a recording screen: one of them is composed of quantons spinning in the direction of the external field, the other in the antiparallel direction. According to the usual theory "the Stern-Gerlach experiment measures σ_z [the z-component of the spin] without changing it" (Bohm 1951 p. 594). However, there is no evidence for this interpretation: all we know about the incoming beam is its composition. One assumes explicitly that its individual components are in states that are linear combinations of the spin eigenfunctions ("up" and "down"). The Stern-Gerlach apparatus orients and separates the spinning quantons; therefore it helps perform a typical obtrusive (or second kind) measurement.

Evidently, the very notion of an obtrusive measurement presupposes the *reality* of the object of measurement as well as the possibility of *distinguishing* it clearly from the measuring apparatus. (A can act upon O only if both A and O are real as well as different.) Yet the standard (or Copenhagen) interpretation of quantum theory denies precisely both the independent reality of quantons and their distinguishability from apparatus and observer. Moreover, that interpretation ignores the obtrusive-unobtrusive distinction, and insists that every measurement act disturbs the object in an uncontrollable manner. According to this opinion even the mere observation of radiative or radioactive decay would be caused or prevented by the presence of observers. This is not what the quantum theories of such processes state, for they acknowledge the existence of spontaneous decays. And it is not what experiment indicates either. Thus, Shimony and three of his students (Hall *et al.* 1977) performed an experiment to ascertain whether the presence of observers does in fact influence the emission of gamma-rays by sodium-22 atoms. The result was negative: there is no psychokinesis – hence no Copenhagen spirit acting on matter. Clearly, then, the continual reference of the Copenhagen theorists to measurement is rhetorical.

Worse, the standard interpretation of quantum theory conflates apparatus and observer, and holds that the frontier ("cut") between the observer-apparatus system, on the one hand, and the quanton, on the other, is conventional: that it can be shifted at will by the observer. (See e.g. von Neumann 1932.) Both conflations are at variance with experimental practice. Here it is essential to isolate the observer from the apparatus (e.g. by using servomechanisms) in order to minimize the former's disturbances on the latter. (Recall Sect. 1.1.) And it is equally essential to distinguish what belongs to the object from what is contributed by the apparatus: the very

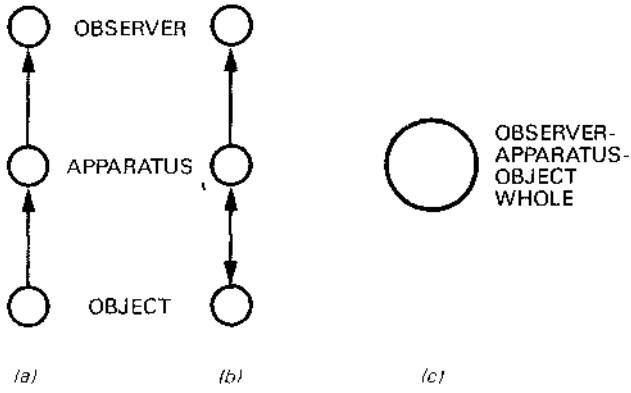


Fig. 2.10. (*a*) Unobtrusive measurement: object send signal to apparatus, which sends signal to observer. (*b*) Obtrusive measurement: apparatus modifies state of object, which sends signal to apparatus, which sends signal to observer. (*c*) Copenhagen interpretation: object, apparatus and observer are indistinguishable except by convention.

notion of an experimental "artifact" (or effect caused by the measuring instrument) rests on the possibility of effecting a sharp distinction between object and apparatus. Neither of the two distinctions is made in the standard quantum theories of measurement (e.g. von Neumann 1932, London and Bauer 1939, Wigner 1963). Both distinctions are made in the realist treatments of measurement (e.g. Everett 1957, Bunge 1967c, Hepp 1972, Machida and Namiki 1980, Cini 1983, Bunge and Kálnay 1983b). The contrast between the actual theory and practice of measurement, on the one hand, and the Copenhagen account of it, is summarized in Figure 2.10.

All the adherents of the Copenhagen interpretation agree that the apparatus is ever-present – a belief refuted by the fact that most of the problems solved in quantum theory concern exclusively quantons (Sect. 4.2). However, the Copenhagen camp is divided on the question of whether a quantum theory of measurement is necessary or even possible. Bohr denied both the necessity and the possibility, arguing that the general theory refers exclusively to experimental situations anyhow, and that the behavior of the apparatus has to be described classically. His favorite disciple conceded that a quantum theory of measurement is possible but insisted that it is unnecessary and warned that it is misleading: "a comprehensive quantal account of the measuring process (which is doubtless possible) is actually pointless (except inasmuch as it would provide a rather trivial check of the consistency of the quantum theory), since it would only obscure the essential function of the

experimental arrangement in establishing the connexion between the quantal system and the classical concepts indispensable for its description" (Rosenfeld, 1964, p. 219).

A number of physicists, even while paying lip service to orthodoxy, have proposed a variety of general quantum theories of measurement. None of these theories has been put to the test, whence there is no objective ground for preferring any of them to the others. All these theories assume that the measurement results are both sharp and error-free, and they are so extremely general that they do not model any real measurement set-ups. We all know that every precision measurement involves some accidental error, and that there are no all-purpose measuring devices. Being so general, none of those theories allows one to design any measuring instruments or to calculate any precise and therefore testable predictions. In particular, they do not tell us how to measure positions and times, although the measurement of any property ultimately boils down to a sequence of position and time measurements. (Current quantum theory cannot solve satisfactorily the problem of position measurement because the position eigenstates are deltas, which are mathematically ill-behaved; the well-behaved position eigendifferentials correspond to position intervals, not points. As for time, it is a classical variable in quantum theory, so the latter has nothing new to say about it.) Such barrenness and remoteness from the laboratory of the general quantum theories of measurement is the more ironic given the apparent commitment of most physicists to operationism.

However, the utter failure of the *general* quantum theories of measurement does not condemn the program of building *specific theories* accounting for particular measurement processes in strictly physical (quantal *and* classical) terms. We need them to understand, and therefore operate correctly, and improve on, our laboratory equipment. (As a matter of fact there are already a number of such specific theories – e.g. of the blackening of photographic plates and of the working of the Geiger-Müller counter. Recall Sect. 1.1.) But we need a great many specific theories rather than a single fits-all theory. And the theories we need must not be purely quantal because all existing measuring instruments are classons, and quantum theory lacks the predicates required to represent some of their properties – e.g. rigidity, temperature, and viscosity. The theories we need are semiquantal or, equivalently, semiclassical. In particular they must account for the amplification process – e.g. in a photomultiplier – that starts in quantum events and results in macrophysical ones. And, of course, they must be realistic in the sense that they must treat the measurement process as a particular physical

process occurring in a purely physical system – the measurement system described in Sect. 1.1. The theories of measurement that assume (rather than prove) that one can write and solve a Schrödinger equation for such a macrophysical system, and thus solve the measurement problem in all generality, are no better than theological doctrines.

Although we demand a multiplicity of specific theories of measurement – one for every type of measuring device – we admit that they share some features. In particular, there is nearly total consensus that they must account for the *projection* of the quanton state function (or "collapse" or "reduction" of the "wave packet") upon measurement. The intuitive idea is simple. (See e.g. Dirac 1958 §10.) If a measuring operation yields a "sharp" result, such as a single number, this must be an eigenvalue of the operator representing the property being measured; and, according to the eigenvalues postulate (Sect. 4.2), that eigenvalue must correspond to an eigenstate of the same operator (assuming non-degeneracy for the sake of simplicity). But then the original state of the quanton, which, according to the superposition principle, was a linear superposition of such eigenstates, must have been projected onto one of the axes of the functional space of the operator. In other words, the measuring apparatus filters out all the eigenstates except the one corresponding to the eigenvalue that is found upon measurement. In misleading pictorial terms: the initial wave packet, composed of many (perhaps all) eigenstates, reduces to one of them. See Figure 2.11.

Example 1 Polarization measurements, by means of a nicol prism in the case of light, or of a Stern-Gerlach apparatus in the case of massive quantons, actually *prepare* quantons in "sharp" states eigenstates), and so

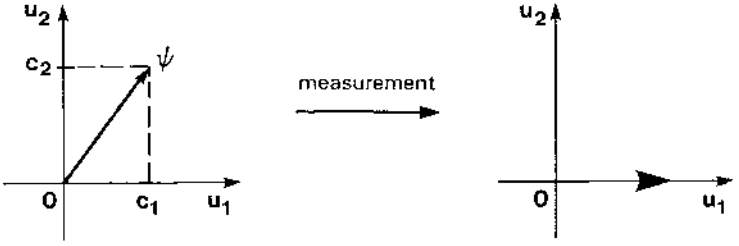


Fig. 2.11. The projection hypothesis: the initial state ψ of the quanton gets projected onto an eigenstate upon measurement (or, more generally, interaction): $\psi = \sum c_k u_k \rightarrow c_i u_i$. The operator effecting this projection is called a projector.

they effect projections. (Caution: only the state functions, not the properties themselves, get projected. For example, if the z-component of the angular momentum, or the spin, of a quanton acquires a definite value upon preparation or measurement, the other two components are blurred and therefore cannot be measured exactly. This follows from the non-commutativity of the angular momentum or the spin components.) *Example 2* Measurements of scattering cross sections involve projections at two stages: when preparing the (nearly) monochromatic incident beam, and when detecting the outgoing quantons. *Example 3* Some wavelength measurements end up in a reduction of the wave at the detector (e.g. the retina).

So far there is nothing problematic about all this except that it involves an idealization, for in many cases actual measurement results are numerical intervals rather than single numbers. (So, a more realistic version of the projection hypothesis is that measurement involves a projection of the original state function onto a subspace, rather than an axis, of the corresponding Hilbert space. But this is a mere technicality.) The real scientific and philosophical problems are posed by the *orthodox* version of the projection hypothesis (von Neumann 1932). Let us recall it briefly:

(i) As long as the quanton is not subjected to observation, its state function evolves smoothly according to the Schrödinger equation. This is called a *type II* process.

(ii) If the quanton is in state ψ before its "observable" (property) A is measured, the observation act throws ψ onto one of the eigenstates of the operator $\hat{A}$ representing A. This instantaneous, discontinuous and uncontrollable change is called a *type I process*.

(iii) It is in principle impossible to predict the precise eigenstate produced by an observation, but we can predict the probability of every possible outcome regardless of the type of measuring device. In fact the probability that ψ collapses onto the particular eigenstate u_k of $\hat{A}$ (Equation (5), Sect. 4.2) equals $|c_k|^2$, where c_k is the coefficient of u_k in the expansion of ψ into the eigenfunctions of $\hat{A}$.

(iv) The projection (reduction, collapse) is not a physical but an epistemological event: since ψ represents only our state of knowledge (not an objective state of affairs), its projection onto u_k represents the observer's change in his state of knowledge upon taking cognizance of the outcome of his observation.

In short, according to von Neumann and his followers there is projection of the state function upon measurement, but it is a purely subjective process

consisting in the sharpening of the observer's information. In other words, *type I processes occur only in the observer's mind*: they do not affect the objects of measurement. (So much so that von Neumann restricted his theory to unobtrusive measurement. Only the observer-apparatus eigenstates were supposed to change as a result of the observation act.) This subjectivist interpretation of the state vector has been found wanting (Sect. 4.2). However, it allowed von Neumann to avoid the embarrassing questions of the mechanism and infinite speed of the "contraction": it did so by displacing them from physics to psychology. Nevertheless the orthodox version of the projection hypothesis posed more problems than it solved.

Firstly, the orthodox version *conflates apparatus and observer* – but we took care of this confusion a short while ago. Secondly, by assuming that the Schrödinger equation holds as long as nobody is observing, the orthodox view renders it *untestable*. (On top of this it contradicts the Copenhagen dogma *Thou shalt not feign any unobservables*.) Thirdly, it renders quantum probabilities *subjective*, and correspondingly turns mean standard deviations (those occurring e.g. in Heisenberg's inequalities) into *uncertainties* caused by the arbitrary or uncontrollable action of the observer-apparatus. (Ironically, this turns quantum mechanics into (*a*) a branch of epistemology, and (*b*) a causal theory – though one with unknowable causes.) Fifthly, by assuming that observation escapes the laws of physics, in particular the Schrödinger equation, the orthodox view treats measurement as an *unphysical* process – which contradicts the practice of measurement as well as the specific (and useful) theories of measurement. Sixthly, contrary to all laboratory experience, it assumes that measurement is *instantaneous*.

If none of the above objections has succeeded in shooting down the orthodox version of the projection hypothesis, think of the following contradiction generated by it (Everett 1973). Consider a quanton Q and an apparatus A by means of which some measurement is performed on Q. According to orthodoxy the projection hypothesis takes the place of the Schrödinger equation during the measurement process. Now consider a second apparatus B capable of observing the system $Q + A$ composed by Q and A. So long as B does not observe this composite system, the latter evolves smoothly according to the Schrödinger equation: it undergoes a type II process even while A is performing a measurement on Q. However, this contradicts the rule directing us to use the projection postulate to describe what goes on in the composite system $Q + A$. If on the other hand the second observer-apparatus B does perform a measurement on the composite system $Q + A$, how is he to describe this process? If $Q + A$ is

treated as a physical system, a concession is made to realism, so the positivist philosophy inherent in orthodoxy is violated. And if the latter is respected, quantum theory leaves us in the lurch, for the projection postulate makes no provision for a second observer-apparatus – much less for a chain of observers, such that observer N observes what observer M is doing, who in turn observes observer L, and so on, down to observer A, who observes Q. So, in either case orthodoxy is untenable.

What is the theorist to do in the face of this disaster? If he is orthodox enough he will swallow the contradiction the way religious believers accept the mysteries of their religion. But if he is capable of independent critical judgment he has the following options: (*a*) to deny that projection occurs, *giving up* the projection hypothesis (Margenau 1936, Everett 1973, Fox 1983); or (*b*) to admit that projection occurs but try and *reform* the projection hypothesis, turning it into an approximate result derivable from the usual physical principles applied to a strictly physical process in which quantons interact with classons. The first solution is unpopular because it is generally recognized that projection does occur in measurement. The second solution is just as unpopular because it calls for the implementation of a difficult research project; therefore some people continue to employ the projection postulate in a pragmatic way, as an *ad hoc* heuristic rule (Lévy-Leblond 1977). We favor stand (*b*) for being principled and for suggesting an interesting research project; more on it in a moment.

(Everett's is the most original and oddest solution to the problem; it ought to appeal to those philosophers who like to play with possible worlds. According to him the state vector does not collapse upon measurement but splits into different branches or "worlds" – and this occurs continuously if the observation is continuous. These "worlds" would coexist and be mutually independent, i.e. non-interfering. But, because they are mutually independent, if we happen to inhabit one of them we will be unaware of all the remaining branches, so that everything *appears* to conform to the projection postulate. For this reason Everett's solution has been called "the many worlds interpretation of quantum theory": deWitt and Graham 1973. This solution to the contradictions generated by the orthodox version of the projection hypothesis is unscientific because the splitting is unobservable, so the conjecture is untestable. We are offered a piece of science fiction in place of a piece of defective science.)

We have been writing about the projection *hypothesis* where von Neumann and his followers wrote about the projection *postulate*. The reason is that we would like to see a rigorous *proof* that the projection, or something

close to it, occurs partly as a consequence of the Schrödinger equation, not as a result of an arbitrary decision of an omnipotent Observer placed above the laws of nature. More precisely, we should like to derive a projection (or semiprojection) *theorem* from physical (quantal and classical) first principles. And we should like to have a proof that the projection (or semiprojection) is a *swift but not instantaneous process caused by certain interactions*, in particular those between quanton and apparatus. Such a proof would restore the unity of quantum theory and eliminate some of the unphysical features inherent in orthodoxy. The projection of a general state function onto one of its components should be just as natural and simple as the landing of a fair coin on one of its "eigenstates": while spinning in space the coin is 50% in the "heads" state and 50% in the "tails" state (Gottfried 1966).

A few physicists have attempted to accomplish just that: to prove projection from physical principles only. Thus Hepp (1972) proved that, in a highly idealized case, the state function of an individual quanton under observation gets reduced in the limit as time approaches infinity. (But, as Bell (1974) pointed out, "$t = \infty$ never comes, so that the wave packet reduction never comes".) More recently, Machida and Namiki (1980) have shown that projection is a result of micro-macro interactions, regardless of either amplification (required for perception) or irreversibility (inherent in recording). However, their treatment concerns whole ensembles of quantons rather than individual ones. Cini (1983) has tackled the problem in a far more realistic way, by studying specific measurements on a single quanton, namely with polarized counters and Stern-Gerlach devices. He shows that the "collapse" occurs to a high degree of accuracy, though not instantaneously, and as a consequence of the basic physical laws. (His treatment is confined to unobtrusive measurements.) Moreover the "collapse" may occur without irreversibility and, what is more important, it is a feature of micro-macro interactions in general, so it is bound to occur not only in the laboratory but throughout nature. This "eliminates that mysterious influence of the observer's mind on the microsystem behavior, which has been at the origin of so much frivolous nonsense by so many famous physicists" (Cini 1983 pp. 50–51). In short, progress is being made in demystifying the quantum theory of measurement and, in particular, in understanding the nature and mechanism of the state function projection.

The importance of research on the deduction and realist reinterpretation of the projection hypothesis can best be gauged by the paradoxes that its orthodox version generates. Only a moment ago we discussed the paradox

constructed by Everett (1973). Another, even more striking, is the so-called *Zeno's quantum paradox*, according to which an unstable system subjected to continual observation does not change, much less decay (Misra and Sudarshan 1977, Chiu *et al.* 1977, Peres 1980, Horwitz and Katznelson 1983). Accordingly, Zeno would be right because Berkeley supposedly is. But at the same time the Copenhagen dogma, that whatever happens is the deed of some observer, would be refuted. Besides, we could all become immortal just by making sure that someone, other than God, is continually watching us.

The paradox stems from orthodoxy (Bunge and Kálnay 1983a, 1983b). It will be instructive to rehearse the paradox, trace its sources, and dissolve it. Consider an unstable quanton, such a free neutron or a radiactive atom, that can be in either of two states or in a superposition of both. Call those states the initial (or excited or undecayed) state, and the final (or ground or decayed) one. The probability that the system be in its initial (excited) state ϕ_1 at time t, either because it stayed in it or returned to it, equals the square of the projection of its state vector ψ onto the initial state ϕ_1, i.e.

$$p(t|0) = |(\phi_1, \psi(t))|^2. \tag{16}$$

For small values of t, a straightforward computation yields

$$p(t|0) \cong 1 - (\sigma/\hbar)^2 t^2, \tag{17}$$

where σ is the mean standard deviation of the quanton hamiltonian. Normally, to obtain the value $p(t|0)$ at any desired instant not too far off in the future one sets t equal to the desired number on the RHS of Equation (17), and no paradox arises.

To produce the paradox we must read Equation (17) in operational terms. Consider the unit interval [0, 1] and divide it into n equal subintervals, observing the quanton at the end of each of them by an unobtrusive means. The probability that the quanton be found in its initial state ϕ_1 at the end of the first time interval $1/n$ is, by Equation (17), $p_1 \cong 1 - (\sigma/n\hbar)^2$. Since by Equation (16) the "survival" probability does not depend upon the previous history of the quanton (hypothesis of statistical independence), after the second observation has been performed we shall have $p_2 \cong [1 - (\sigma/\hbar)^2]^2$. Finally, after the unit interval n/n, the probability of finding the quanton in its undecayed state is

$$p_n \cong [1 - (\sigma/n\hbar)^2]^n, \tag{18}$$

which approaches 1 as n approaches infinity. That is, the quanton stays in

its initial state regardless of the length of the time interval, provided it is being observed all the time. The root of the paradox is obvious: every time an observation is made, even if it is an unobtrusive one, the state vector ψ of the quanton is assumed to project onto its initial state ϕ_1 in obedience to von Neumann's projection postulate. If the observations are made continuously, the quanton is prevented from changing because its state vector is forced to lie down on the initial state all the time: it cannot raise its head because Big Brother is watching. We proceed to show that Zeno's quantum paradox does not occur in our realist version of quantum theory.

By hypothesis the observation on our unstable quanton is unobtrusive (of the first kind). Hence it cannot affect the state of the quanton, which evolves spontaneously according to the Schrödinger equation. But, since the quanton is unstable, its initial state ϕ_1 cannot be an eigenstate of the energy operator: if it were, the quanton would not decay. The clue to the problem is the superposition principle, which tells us that, from the instant it is born – i.e. for all $t > 0$ – the unstable quanton is in a *superposition* of its undecayed (initial) state ϕ_1 and its decayed (final) state ϕ_2. In other words,

$$\psi(t) = c_1(t)\phi_1 + c_2(t)\phi_2, \tag{19}$$

where c_1 and c_2 are the projections of ψ onto the initial and the final states respectively, i.e.

$$c_i(t) = (\phi_i, \psi(t)), \quad \text{for} \quad i = 1, 2,$$

and

$$|c_1(t)|^2 + |c_2(t)|^2 = 1 \quad \text{for all} \quad t. \tag{20}$$

As we saw in Sect. 4.2, $c_i(t)$ is the weight of the contribution of the sharp state ϕ_i, at time t, to the quanton state ψ.

Equation (20) suggests a simple geometrical representation that brings out the profound differences between quantons and classons. Pretending that the coefficients of the superposition (19), are real numbers, and using Equation (17), it is easily seen that the contributions of the sharp states evolve in time according to

$$c_1^2(t) = \cos^2 \omega t \cong 1 - (\sigma/\hbar)^2 t^2, \quad c_2^2(t) = \sin^2 \omega t \cong (\sigma/\hbar)^2 t^2. \tag{21}$$

The physical interpretation of these equations is obvious: see Figure 2.12. As time goes by, the state vector ψ rotates with angular velocity $\omega = \sigma/\hbar$ about the origin of the state space formed by the axes ϕ_1 and ϕ_2. In other

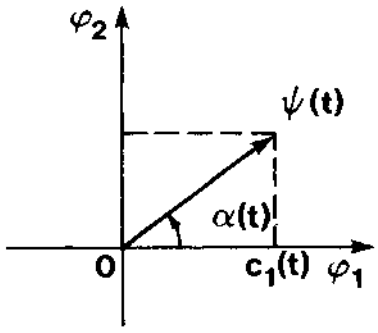


Fig. 2.12. Quantum Zeno's paradox. The unstable quanton is not always either in its initial (undecayed) state ϕ_1 or in the final (decayed) state ϕ_2. Most of the time it is in a linear combination of the two with time-dependent coefficients. The system goes back and forth between the two sharp states.

words, the quanton goes back and forth, in a continuous manner, between the two sharp states every $\hbar/\sigma$ seconds. In the classical case there is no rotation because the energy remains sharp at all times, i.e. $\sigma = 0$. As long as the quanton energy is spread ($\sigma \neq 0$), the quanton does not stay put in any energy eigenstate.

In conclusion, quantum Zeno's paradox does not arise if the projection hypothesis is not interpreted in a subjectivist manner, and the superposition principle is taken seriously. More on the second point in the next subsection (taken from Bunge 1984a).

6.2. *Hidden Variables, Separability, and Realism*

In the section we will discuss some developments that originated with the famous paper by Einstein, Podolsky and Rosen (1935) – henceforth EPR – and culminated with the experimental tests of hidden variables theories (in particular Aspect *et al.* 1981, 1982). In that paper it was held that the current quantum theory is incomplete for being about ensembles of similar things rather than about individual entities. (We criticized this view in Sects. 4.2 and 5.2.) On this view individual microphysical objects would have precise though different positions, velocities, energies, etc. Randomness would not be a basic mode of being but an appearance originating in individual differences among the components of an ensemble of mutually independent things. (Think of the distribution of bullet impacts around the bull's eye of a target.) A complete theory should contain hidden (classical)

variables only, and it should deduce all the probability distributions instead of postulating them.

A hidden variable is a function possessing definite or sharp values all the time instead of being spread out: it is *dispersion-free* like the classical position and the classical field intensities. This type of variable was called 'hidden' in contrast with the so called "observables" and for allegedly referring to a sub-quantum level underlying that covered by quantum theory. (See Bohm 1957 for a fascinating account.) It was hoped that quantum theory could be enriched or completed with hidden variables, or even superseded by a theory containing only variables of this type. Bohm's theory (1952), which revived and expanded de Broglie's ideas on the pilot wave, was of the first kind. Stochastic quantum mechanics (de la Peña-Auerbach 1969) and stochastic electrodynamics (e.g. Claverie and Diner 1977) are of the second type. Moreover the latter theories, far from being thoroughly causal, contain random hidden variables like those occurring in the classical theory of Brownian motion.

Hidden variables theories were supposed to accomplish five or six tasks at the same time. It will pay to list and examine them separately because they are often conflated, with the result that there is still no consensus on what exactly experiment has refuted. Those tasks are:

(i) To *restore realism in the philosophical sense of the word*, i.e. to supply an objective account of the real physical world, rather than describe the information, expectations, and uncertainties of the knowing subject.

(ii) To *restore realism in EPR's idiosyncratic sense of the word*. That is, to comply with the oft-quoted EPR criterion of physical reality: "*If, without in any way disturbing a system, we can predict with certainty (i.e., with probability equal to unity) the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity*" (Einstein *et al.*, 1935, p. 777).

(iii) To *restore classical determinism* by deducing chance from causality – i.e. to explain probability distributions in terms of individual differences and the mutual independence of the components of statistical ensembles.

(iv) To *complete* the job begun by quantum theory, regarded as a statistical theory, by accounting for the (determinate) behavior of individual physical entities.

(v) To replace quantum theory , regarded as a phenomenological (or black box) theory, with a *mechanismic* theory that would explain quantum behavior instead of just providing successful recipes for calculating it. I.e., to exhibit the mechanisms underlying quantum behavior – e.g. electron

diffraction and the tunnel effect –, deducing quantum theory as a particular case, in some limit, or for special circumstances.

(vi) To *restore the separability* or independence of things that, having been components of a closely knit system in the past, have now become widely separated in space – i.e. to eliminate the distant or EPR correlations. More on this below. Suffice it now to recall that, according to quantum theory, once a (complex) system, always a system (Vol. 3, Ch. 5, Sects. 4.1 and 4.2).

The first objective – the restoration of philosophical realism – was legitimate. However, it can be attained without modifying the mathematical formalism of quantum theory and, in particular, without introducing hidden variables. Indeed it can be shown that the Copenhagen interpretation is adventitious and that the realist interpretation, which focuses on things in themselves rather than on the knowing subject or observer, is not only possible but the one actually employed by physicists when not in a philosophical mood: recall Sects. 4 and 5. (For details see Bunge 1967c and 1973a.)

The second goal, though often mistaken for the first, has nothing to do with philosophical realism: it is simply *classicism*, and it involves the denial of the superposition principle, according to which the dynamical properties of a quanton normally have distributions of values rather than sharp values. (In the non-relativistic theory only the mass and the electric charge have sharp values all the time – but these are not dynamical variables.) In the light of the indisputable success of quantum theory and the failure of hidden variables theories, that classicist principle must be regarded as unjustified: we shall call it the *EPR dogma*. Paradoxically enough, this dogma can be upheld by surrendering classical logic. Indeed Friedman and Putnam (1978) have suggested that the variables of quantum mechanics can be regarded as "quantities [that] *always* have well-defined values" provided the distributive law for propositions is given up. This procedure has the advantage that it also restores realism in the philosophical sense (i). The catch is that the resulting theory bears only a superficial similarity to the quantum theory of physicists, which has ordinary logic built into it; in particular, this version of quantum theory yields the wrong probabilities: recall Sect. 5.2.

The third objective – determinism – was pursued by the early hidden variables theorists, but was no longer an aim of those who worked from about 1960 on. In fact, as we saw a while ago, there are theories containing random hidden variables, namely stochastic quantum mechanics and stochastic quantum electrodynamics. The introduction of these theories was important to distinguish the concepts of (i) philosophical realism, (ii) classi-

cism or EPR realism, and (iii) determinism – as we realize with hindsight.

The fourth goal (completeness) makes sense only if quantum theory is regarded as a statistical theory dealing only with ensembles. It evaporates as soon as it is realized that the elementary theory refers to individual quantons (Sects. 4 and 5).

The fifth objective (mechanismic theory) was soon abandoned in the most advanced work on hidden variables: that of Bell (1964, 1966). In fact Bell's hidden variables have no precise physical interpretation: they are only adjustable parameters in a system that is far more phenomenological than quantum theory. Moreover such hidden variables are not subject to any precise law statements, hence they are no part of a physical theory proper. But this is a virtue rather than a shortcoming, for it shows that Bell's famous inequalities hold for the entire *family* of local hidden variables theories, regardless of the precise law statements that may be assumed.

So far, only the first objective – namely the restoration of philosophical realism – seems worth being pursued. However, it can be attained without the help of hidden variables, namely by a mere change of interpretation of the standard formalism of quantum theory. On the other hand the sixth objective, namely separability, remained plausible for about half a century. Indeed, the EPR distant correlations are quite counter-intuitive. How is it possible for the members of a divorced couple to behave as if they continued to be married, i.e. in such a manner that whatever happens to one of them affects the other? The quantum-theoretical answer is simple if sybilline: *there is no divorce*. However, let us not rush things: it will pay to have a look at the experiment suggested by Einstein *et al.* (1935). Here we shall deal with an updated version of it in the context of Bell's general hidden variables theory, which contains the now famous Bell's inequalities.

According to quantum theory the three components of the spin of a quanton do not commute, hence (in our interpretation) they have no definite or sharp values at the same time – as a consequence of which they cannot be measured exactly and simultaneously. Simpler: the spin components are not the components of a vector, hence there is no vector to be measured. On the other hand, if the spin were an ordinary vector (hence a hidden variable), its components would commute and consequently it should be possible to measure all three components at the same time with the accuracy allowed by the state of the art.

Apply the preceding considerations to a pair of things that are initially close together and with spins pointing in opposite directions, and subse-

quently move apart without being interfered with. Compute the probability $P(x, y)$ that thing 1 has spin in the positive x-direction, and thing 2 in the positive y-direction. According to quantum mechanics this probability exceeds the sum of the probabilities $P(x, z)$ and $P(y, z)$:

$$P(x, y) \geq P(x, z) + P(y, z). \qquad QM$$

On the other hand any hidden variables theory predicts the exact reversal of the inequality sign:

$$P(x, y) \leq P(x, z) + P(y, z). \qquad HV$$

This is one of Bell's inequalities. Bell himself and others derived several other inequalities, holding in all local hidden variables theories, and involving exclusively measurable quantities such as coincidence counter rates. (See Clauser and Shimony, 1978 for a masterly review.)

Thanks to this theoretical work a number of ingenious and highly accurate measurements were designed. Some of them, involving photons rather than massive quantons, have been performed. The latest version is shown schematically in Figure 2.13. An atom at S emits two photons at the same time, and their linear polarizations are measured by the polarizers I, oriented in the direction a, and II, oriented in the direction b. (These directions are externally adjustable.) The photons coming out of the polarizers activate the photomultipliers PM1 and PM2. The coincidence rate is monitored by the

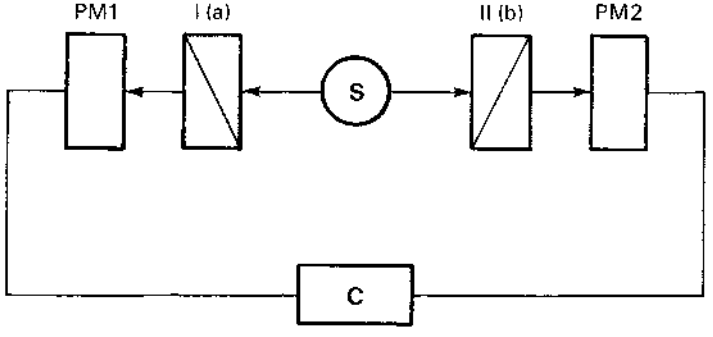


Fig. 2.13. Updated optical version of the EPR experiment. S: source. I(a) and II(b): polarizers (analyzers). PM1 and PM2: photomultipliers (amplifiers). C: coincidence counter activated only by the simultaneous arrival of signals from the two analyzers. Adapted from Aspect *et al.* (1982). Think not of two distinct things arriving at I and II but of a *single* system expanding up to I and II. The system breaks down when striking the photomultipliers PM1 and PM2.

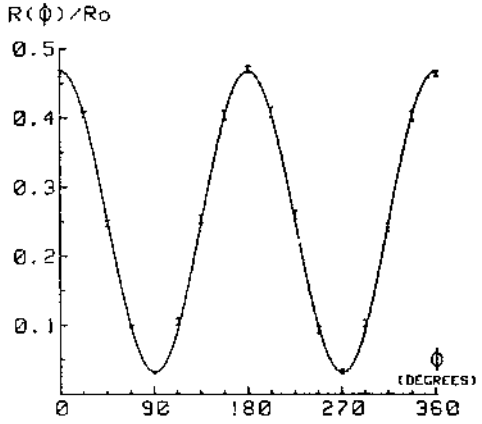


Fig. 2 14 The result of the EPR optical experiment diagrammed in Fig. 2.13. Coincidence rate as a function of the relative orientations of the polarizers. The data fit the quantum theoretical prediction (solid curve) almost to perfection. Taken from Aspect *et. al.* (1981).

counter C. The result is that the polarization states of the two photons are highly correlated: see Figure 2.14. That is, the state of polarization measured by polarizer II depends on that measured by polarizer I and conversely (Aspect *et al.* 1981). Essentially the same result obtains if the polarizers are rotated during the flight of the photons and before they strike the polarizers, to prevent any communication between them through signals propagating at a finite speed (Aspect *et al.*, 1982).

Although there is consensus that Bell's inequalities, and with them all of the hidden variables theories, have been experimentally refuted, it is not clear *what is at issue*: philosophical realism, determinism, the EPR dogma that all properties have sharp values at all times, or the EPR hypothesis that distant things behave independently of one another. (See Table 2.1.) Unfortunately most authors (e.g. Clauser and Shimony 1978, d'Espagnat 1979, Aspect *et al.* 1981) claim that *realism* is the casualty, without however clarifying what they mean by 'realism', and ostensibly conflating philosophical realism with what we have called 'classicism' or the 'EPR dogma'. Yet from our discussion in previous sections it should be clear that the very design and performance of measurements presuppose the reality (independent existence) of the entire measurement system, hence of every component of it (object, apparatus, and experimenter). So, philosophical realism is definitely not at issue: the downfall of the Bell inequalities has not refuted

the principle that the physical world manages to exist without the help of those who try to know it. D'Espagnat (1980), when cornered by Michel Paty, has admitted this. So has Shimony (personal communication 1982).

TABLE 2.1. Terminological guide for the perplexed.

Expression	In this work	In the physics literature
system	system (thing with at least two components)	**1**: system **2**: physical object (simple or complex)
observer	observer	**1**: observer **2**: apparatus **3**: observer-cum-apparatus
realism	the thesis that nature exists by itself	the thesis that all properties have definite or sharp values all the time
classicism or EPR dogma	the thesis that all properties have definite or sharp values all the time	realism
hidden variables	dispersion-free functions	**1**: dispersion-free functions **2**: non-stochastic variables
Determinism	**1**: lawfulness plus *ex nihilo nil* **2**: causality	**1**: causality **2**: non-probabilistic lawfulness **3**: individual predictability
predictability	predictability	determinism
locality	nearby action	**1**: nearby action **2**: separability

Nor is determinism at issue, since some hidden variable theories entailing Bell-type inequalities contain random variables. (See e.g. Stapp 1980.) So, *only the EPR dogma and the separability (locality) hypothesis* are at issue. In fact this is the conclusion Clauser and Shimony (1978) reach after their careful analysis: Hidden variables (sharpness) & Locality (separability) ⇒ Bell's inequalities. Hence the experimental refutation of these inequalities refutes either separability or the EPR dogma – or both. We shall argue in a while that the *or* is inclusive, for the EPR dogma entails separability ("locality"). But before doing this we must have another glance at the Aspect *et al.* (1981, 1982) experiments.

The experiment poses two problems. One is to ascertain whether the quantons emitted by the source possess all their properties, in particular polarization, or acquire them when interacting with the polarizers. The second problem is to account for the correlations at a distance, or EPR correlations, between the results obtained with analyzer I and those obtained with analyzer II – or, equivalently, to explain the coincidences registered by the coincidence counter.

Let us start with the first problem. According to both classical and hidden variables theories, the fragments resulting from the process at the source have sharp properties (eigenvalues) from start to finish: the analyzers only exhibit what is already there. On the other hand, according to quantum theory the quantons leaving the source are in a superposition of polarization states. This superposition collapses (reduces) to eigenstates through interaction of the system with the analyzers. In other words, the analyzers do not just measure an existing polarization but also produce it: they are measuring instruments of the second kind. Clearly, this explanation will satisfy only those who believe that accurate measurement is accompanied by a reduction of the state function – even though one need not believe the dogmas that this reduction is subjective, instantaneous, and unrelated to the smooth processes described by the Schrödinger equation. (Recall Sect. 6.1.)

The second problem is this: If only one of the analyzers is read, we may infer the result obtained with the other analyzer without looking at it. (This cannot be done with the apparatus shown in Fig. 2.13, which registers only coincidences. The equivalent operation here is to alter the angle between the analyzers and note the change in coincidence rate. As shown in Figure 2.14, the latter attains its maximum when the two analyzers are parallel.) This result is nicely explained by any hidden variables theory jointly with the theorem of conservation of the total spin. Indeed, the first quanton would arrive at the analyzer possessing a definite spin value, say 1, so that the second quanton must have spin -1 since the total spin of the system at the source was 0). The trouble with this explanation is that it takes hidden variables for granted. Since experiment has refuted hidden variables theories we must try to explain the same results with the help of quantum theory.

If the two quantons leaving the source were to become independent after a while – as they should according to classical physics – the result obtained with analyzer I should be independent of the result obtained by analyzer II. Thus is should be possible to observe, say, the left quanton with its spin pointing in the x-direction, and the right quanton with its spin pointing in

the z-direction. But this is not what quantum theory predicts: according to this theory there is total spin conservation, so that if the left analyzer projects the spin onto the positive x-axis, the right analyzer will project it onto the negative x-axis. In other words, there is a strong correlation at a distance between the polarization of the two former components of the original system. This correlation is correctly predicted by quantum theory, which treats the "two" quantons as *one*. (Only the state function for the entire system satisfies the Schrödinger equation.)

The quantum-theoretical explanation of distant correlation is often found unsatisfactory or even mysterious because it involves the counter-intuitive ("paradoxical") hypothesis that a complex system may continue to be such even after its components have moved far apart. In fact all known classical forces fall off quickly with increasing distance. For that reason a host of more or less unorthodox explanations of the EPR distant correlation have been proposed. Among them we note those in terms of hidden variables and a peculiar quantum potential that does not occur in the hamiltonian but derives from the state function of the system (Bohm and Hiley 1975), the presence of actions at a distance and even psychokinesis. We reject all three explanations for the following reasons. The first occurs in a hidden variables theory, hence one containing some of Bell's false inequalities. The second violates special relativity and electrodynamics (both classical and quantal), which involve the principle of nearby action (or locality in the field-theoretic sense). And the third violates the principle of conservation of energy, since every message must ride on a physical process – unless, of course, one is prepared to believe in ghosts.

We need not look for any special *mechanisms* explaining the distant or EPR correlations, any more than we need to explain the Lorentz length "contraction" and time "dilations" in terms of mechanisms (Paty 1981). Quantum mechanics accounts for distant correlations as follows. If two quantons are initially independent from one another, then the state function of the aggregate they constitute is correctly represented by the product of their individual state functions. So, separability or "locality" obtains in this case just as in classical physics. But if two quantons have initially been part of a system, i.e. if they have interacted strongly at the beginning, then the state function of the whole cannot be so factorized even after the components have moved far apart. In other words, the state of each component is determined not only by the local conditions (i.e. the state of the immediate surroundings of the spatially separated quantons), but also by their still belonging to a system. So, although physical separation entails spatial

separation, the converse is false. Quantum theory is then *non-local* in this peculiar sense or, as we prefer to say, it is *systemic* in that it does not incorporate the classical principle that widely separated things cannot belong to the same system. (Note the ambiguity of the word 'local'. In field physics, whether classical or quantal, 'locality' means that nearby action, or non-action-at-a-distance, obtains: all actions are assumed to proceed *de proche en proche*. Quantum theory *is* local in this traditional sense of the word.)

We submit that the non-separability (or non-"locality") inherent in quantum theory is a consequence of the superposition principle together with the Schrödinger equation (Sect. 4.2). In fact, once a system is in a state consisting in an inextricable merger (not just either a sum or a product but a sum of products) of the states of its components, the Schrödinger equation guarantees that the system will evolve in time in such a way that its state function will keep that structural property even though the relative contribution of every individual eigenstate is likely to change in the course of time. (More precisely, consider a system with components I and II, and a property M that, like the z-component of the spin, can take only one of two values, m_1 or m_2, on each quanton at a time. Call u_1 and u_2 the states of component I in which M takes on the values m_1 and m_2 respectively, i.e. such that $Mu_1 = m_1u_1$ and $Mu_2 = m_2u_2$. Similarly, call v_1 and v_2 the corresponding states of component II. Then the total system I + II is in some such state as $2^{-1/2}(u_1v_1 + u_2v_2)$ or $2^{-1/2}(u_1v_1 - u_2v_2)$. If the u's and v's are spin eigenstates, the preceding functions are distance-independent: the components continue to be entangled no matter how far apart they move.)

The EPR *paradox* is such only if one presupposes the *classical* principle that the behavior of things that are far apart cannot be correlated. (See Einstein, 1948 and Bell, 1966 for statements and discussions of this classical principle of "locality".) The physicist who takes this principle for granted

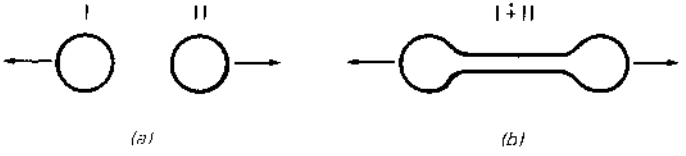


Fig. 2.15. The EPR experiment. (*a*) Classical account: two mutually independent things emerge from the source (*b*) Quantum-theoretical account: the source emits a single system, the state function of which occupies a region that is in one piece.

is bound to puzzle over how it could be possible for two things to continue interacting after they have stopped being components of a system. Quantum theory solves the "mystery" much in the same way as a detective solves an alleged murder case by proving that there was no murder to begin with: that the system was not dismantled, that the distant components are still parts of the original system: see Figure 2.15. What the quantum theorist may puzzle over is a different question, namely: How, that being the case, could one effectively *dismantle* the system, and thus factorize its state function. We submit that the answer to this query is: The original system becomes dismantled only when at least one of its original components gets integrated into another system – e.g. when it is captured or absorbed by an atom.

In conclusion, (*a*) when two quantons interact, their state functions become entangled (not factorizable); (*b*) when the two quantons separate widely in space, they continue to form part of the original system although they do not act upon one another, much less at a distance and instantaneously (d'Espagnat 1981); (*c*) spatial separation is no cause for divorce: there is divorce only if there is new marriage; (*d*) non-separability is a consequence of the superposition principle and the Schrödinger equation; (*e*) non-separability is possibly "*the* characteristic trait of quantum mechanics" (Schrödinger, 1935b p. 555); (*f*) the failure of classical separability or "locality" (Einstein separability) confirms the systemic world view (Vol. 4) – not however the holistic one, because we do succeed in *conceptually* analyzing the composition and structure of systems; (*g*) in quantum theory there is EPR distant correlation (or EPR effect) but there is no paradox: the paradox arises only if quantum theory is combined with the classical principle of separability or "locality".

Philosophers have been quick to exploit the downfall of Bell's inequalities. Thus Fine (1982) has interpreted it as a refutation of determinism, and van Fraassen (1982) as a refutation of epistemic realism. We have seen that it is neither. In fact some hidden variables theories, such as stochastic quantum mechanics, are non-deterministic in that they contain random variables. (See additional criticisms by Nordin 1979 and Eberhard 1982.) As for epistemological realism, it would indeed be dead if, as van Fraassen contends, it involved the hypothesis that there is a causal mechanism underlying every correlation, since no such mechanism is known to exist in the case of the EPR distant correlations. But such a version of epistemological realism is a straw man. Epistemological realism happens to be an epistemological not an ontological view, and it boils down to the thesis that nature exists even if it is neither perceived nor conceived. (Consult any

philosophical dictionary or recall Vol. 6, Ch. 15, Sect. 2.) Moreover, epistemological realism is compatible with alternative ontologies, in particular neodeterminism, which acknowledges non-causal (in particular probabilistic) types of determination (Bunge 1959a).

If anything, the experimental refutation of Bell's inequalities, like any other experiment, has confirmed epistemological realism once more, since every well designed experiment involves a clear distinction between knowing subject, object of knowledge, and apparatus. (Recall Sects. 1.1. and 6.1. See also Wheeler 1978 and Rohrlich 1983.) Only "realism" in the idiosyncratic sense of the EPR paper has been refuted along with Bell's inequalities – but that was only a classicist dogma. Actually this dogma went down the moment quantum theory succeeded in solving the problems that classical physics had solved incorrectly or was incapable of formulating. Since then we have learned that reality is smudged rather than neat: that every quanton possesses some properties that, far from having sharp values all the time, have distributions of values; and that, unlike rigid bodies but rather like fluids and fields, quantons are extremely sensitive to their environments, whether natural or artificial. This discovery did not alter nature and, in particular, it did not enslave nature to the observer. It only taught us that nature is composed not only of classons but also of quantons and semiquantons. It altered our representation of the world, not the world itself: it was a scientific revolution, not a cosmic cataclysm.

Hidden variables theorizing remained outside the mainstream of physics until Bell and others succeeded in deriving formulas capable of being subjected to rather direct crucial experimental tests. Then it came briefly to the fore, and it may soon become little more than a historical curiosity. However, the failure of hidden variables theories has taught us a valuable lesson: that, like other revolutionary theories before, quantum theory has got to be understood in its own terms. (See Bunge 1967c and 1973a.) Besides, hidden variables theorists have rendered a valuable service to the scientific and philosophical communities. Firstly, they have criticized some of the inconsistencies and obscurities of the Copenhagen orthodoxy. (Unfortunately they have often confused the issues – e.g. the problem of objectivity with that of determinism. See Bunge 1979c.) Secondly, they have helped others remove those inconsistencies and obscurities by reinterpreting the standard mathematical formalism of quantum theory in a strictly physical, hence realist, fashion. Thirdly, they suggested the very experiments that were to refute classicism. Now that the hidden variables episode seems to be closed for good we can look forward to theories even less intuitive than

quantum theory. One wonders whether Einstein was right in believing that God (= Nature) is not malicious.

In a last ditch effort, the anti-realist might look elsewhere for support – and he is likely to find it in a number of carelessly worded texts. In particular he may claim that quantum theory postulates (*a*) a number of *unobservables* – and how could these bolster epistemological realism?; (*b*) that quantons of the same kind (e.g. electrons) are *indistinguishable* – and how can we make sure that there are two things out there if we are unable to distinguish them?; and (*c*) that quantons are nothing but the embodiments of *symmetries*, which would be Platonic ideas. Let us meet these objections.

It is true that quanton theory – like every other fundamental theory – includes a number of concepts lacking observational counterparts. Stationary states (in particular ground states) and virtual particles are cases in point. However, they are not of the same kind. The occurrence of the former only shows that quantum theory is unfaithful to operationism. (Realism holds that reality is necessary though insufficient for observability. Only positivism equates existence with observability.) As for the so-called virtual entities and processes, they are horses of a different color – or, rather, no horses at all. For example, two electrons are said to interact via a virtual photon, and two quarks via a virtual gluon. Such entities and processes cannot be real because they violate certain physical laws, notably energy conservation. Consequently they are admittedly unreal (virtual) and therefore unobservable – and yet they are postulated by scientists who pay lip service to operationism. Unlike stationary states, potentials, and other concepts with real counterparts, virtual things and processes are dispensable fictions (Bunge 1970). They have been invented only because some physicists, particularly Feynman, insist on attributing a physical (and if possible pictorial) interpretation to every term in a formula and every line in a diagram (Bunge 1955b). Only the naive realist demands a one-to-one correspondence between the concepts of a theory and "elements of reality" (an EPR expression). Scientific realism demands only that there be a whole theory-whole thing correspondence (Vol. 1, Ch. 3, and Vol. 6, Ch. 15). In short, unobservables, in particular virtual things and processes, can offer no comfort to the anti-realist.

As for indistinguishability, it is usually said that, if two particles of the same kind (e.g. electrons) are components of a system, then they are identical and therefore indistinguishable. (See Figure 2.16.) The unwary philosopher might conclude the refutation of Leibniz's principle of the identity of indiscernibles. But a careful analysis of the formulas in question

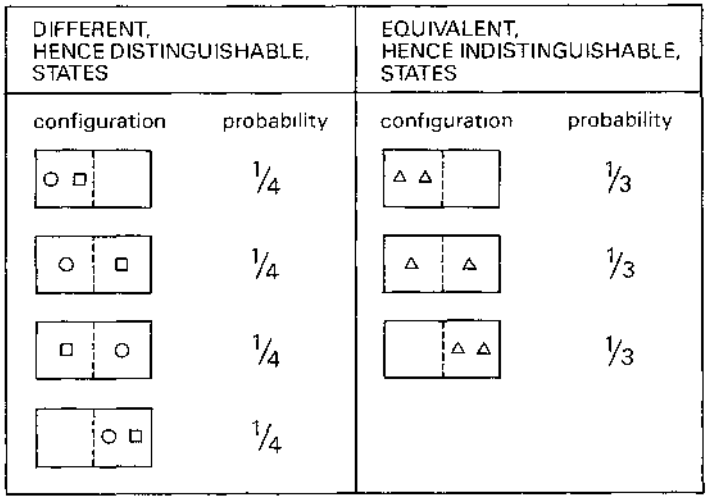


Fig. 2.16. "Indistinguishable" (exchangeable) components of a system. The state spaces of the system will differ or not according as the states "object 1 in cell 1 and object 2 in cell 2", and "object 1 in cell 2, and object 2 in cell 1", are assumed to be the same or different.

reveals that the physicist means *equal* or *equivalent* when he writes 'identical', and *exchangeable* when he says 'indistinguishable'. Indeed he assumes that the particles in question are different since he speaks of *two* or more entities, not of one. (When constructing the state function, e.g. the Slater determinant, of the system, he starts by writing '$N!$' in the denominator, where N is the number of particles in the system – and he trusts that some experimenter will be able to count them, i.e. to determine the actual value of N.) What happens is that an exchange of such quantons makes no difference to the state of the system. In other words, the correct formulation of the principle is: "The state of a system of quantons of the same species is invariant under permutations of its components". Once again, no comfort for the anti-realist.

Finally, some physicists – notably Heisenberg (1969) – have stated that symmetry precedes existence: that quantons are nothing but embodiments of symmetries. This Platonic delusion stems from the manner the theorist confronts the bewildering array of "fundamental particles". Instead of proceeding inductively, or else by trial and error, he imagines that there is a single basic quanton that can be in different mass, charge, spin, isospin, hypercharge, etc., states. He then forgets for a while that hypothetical entity and investigates the algebraic properties of its state space, guided only by

extremely general physical principles. In particular, the theorist investigates the group-theoretic structure of that space: he conjectures, say, that the structure is an US(2), US(3), or some other symmetry group. Once he is done he glances at the "particles" chart and, if lucky, discovers that nature does contain "embodiments" of such conjectured symmetries; and if very lucky, he will predict one or two still unknown "embodiments". But this is no evidence for the power of pure mathematics to mirror the world: all such symmetry groups are constructed on the basis of law statements such as commutation formulas for dynamical variables. Nor do symmetries hover over things, let alone produce them; in physics every symmetry is a property of either a physical entity, such as a crystal, or of a feature of a physical entity, such as the hamiltonian or the state function of a molecule. Melt the crystal, or dissociate the molecule, and the corresponding symmetry disappears.

In conclusion, quantum theory accounts for quantons as things quite different from classons, and therefore it offers a rather counterintuitive ("paradoxical") picture of reality. However, this picture is consistent, extremely rich and fertile, and perfectly compatible with epistemological realism. Inconsistencies, obscurities, paradoxes and uncertainties appear only when trying to force the theory into either the classical or the positivist mold.

7. Chemistry

7.1. *Philosophy and Chemistry*

We define chemistry as the scientific investigation of chemosystems and their molecular products. (In turn, a chemosystem was defined in Vol. 4, Ch. 2 as a system wherein chemical reactions, such as $H + H \rightarrow H_2$, occurs.) Because chemosystems are composed of physical things, and are in turn often components of biosystems, chemistry lies between physics and biology. More precisely, physics helps explain chemistry, which in turn helps explain biology. This well known fact, along with the remarkable success of chemists in accounting for the marvelous variety of substances – millions of kinds of them – in terms of atoms of only about 100 kinds, and the stunning success of chemistry as the scientific basis of the powerful chemical industry, accounts for the popularity of chemistry since the mid-19th century.

Given the popularity and prestige of chemistry, it is strange that the

corresponding philosophy hardly exists. The publications in this field are only a handful (e.g. Prélat 1947, Caldin 1961, Theobald 1976, Lévy 1979, Bunge 1982b and Vol. 4, Ch. 2, Primas 1983). Not even the distinguished philosophers of science Meyerson, Broad and Bachelard, who started out as chemists, made any significant contributions to the philosophy of chemistry: they preferred to write about other sciences. One cause for the underdevelopment of the philosophy of chemistry is that the vast majority of chemists are experimentalists impatient with foundational or even theoretical issues – to the point that theoretical chemistry constitutes a rather small and slowly growing body of knowledge. A second cause may be the philosophical misconception that chemistry raises no philosophical problems of its own for allegedly being but an application or even a logical consequence of physics.

Be that as it may, the philosophy of chemistry is nearly virgin soil that deserves being cultivated vigorously because it contains a rich and important problematics. The short and haphazard list that follows should confirm this statement. What can ontology learn from chemistry with regard to qualitative change, in particular emergence, and the intertwining of cooperation with competition in the chemical formation and breakdown of systems? Are the various laws of chemical reactions (around 6 million of which are known) mutually independent, or just instances of a single general law – or neither? How realistic are the ball and spoke models of molecules? Given that modern chemistry is based on atomic physics, does it follow that it is but an application of the latter, or does chemistry have concepts and hypotheses of its own? In this section we shall deal only with a small sample of the problematics of the philosophy of chemistry.

A first surprise that the chemical literature has in store for the philosopher is the frequency with which teleological and even anthropomorphic expressions occur in it. Examples: "The dissociation energy of a molecule is the energy *needed* to dissociate it", "An endothermal reaction is one that *requires expenditure* of energy to keep it going", "The energy that *must be supplied* to a molecule *in order to* break a bond is called the 'activation energy'", and "High energy bonds are enzymatically broken down to yield the energy *needed* to drive biosynthetic reactions".

Such expressions originate probably in the pragmatic attitude natural to the bench chemist and, particularly, to the industrial chemist. They are quite harmless in basic chemistry, for no chemist dreams of using them to explain anything: they are just sloppy descriptive phrases, and they have a legitimate place in chemical technology (industrial chemistry and chemical engineer-

ing). Moreover, if pressed any chemist is willing and capable of translating any such phrase into a rigorous scientific formula, e.g. "The dissociation energy of a molecule is the minimal energy that dissociates it". However, those teleological and anthropomorphic expressions can do great harm when exported to biology, which is still in the grips of ancient finalism. Whence a first task for the philosopher of chemistry: that of policing the language of the science.

Even deeper metaphysical trouble awaits the philosopher intent on examining the most innocent-looking chemical formulas, such as the familiar stoichiometric formula "NaCl" and the common reaction formula "$H + H \rightarrow H_2$. These formulas are of the types

$$AB \tag{22}$$

and

$$A + B \rightarrow C \tag{23}$$

respectively. In either case one may read them either platonically or nominalistically. In the former case one presupposes the reality of universals, in this case of chemical species; in the latter case one assumes that only individuals are (physically) real. Indeed, (22) may be read either as "Compound AB is composed of species A and B", or "Every individual of species AB is composed of one unit of species A and another of species B". Likewise (23) may be read either as "Species A and B combine to produce species C", or "One unit of species A combines with one unit of species B to produce one thing of species C".

In most cases this double talk is harmless. But when it comes to the foundations of chemistry the difference in interpretation will suggest alternative research lines and consequently different theoretical results. The Platonic interpretation suggests considering every chemical as the resultant of vectors in a vector space the base of which is constituted by the chemical elements (species), and a chemical reaction as a transformation of vectors of this kind (Aris 1965). On the other hand the nominalistic interpretation suggests forming a very different vector space, namely the state space of the system; here the axes are the concentrations of the various chemicals involved, be they elements or compounds (Vol. 4, Ch. 2).

Are chemical equations merely descriptive or do they explain as well? They are supposed to discharge both functions. (See Vol. 6, Ch. 10, Sect. 1.1 for the differences between these two operations.) However, chemical equations are only components of descriptions and explanations.

Strictly speaking, the mere existence of a compound or of a reaction calls for a whole special theory (or model). This is sometimes overlooked by experimental chemists, who are often content to describe a compound, find out its atomic components, and describe one of the reactions that produce it. Therefore the philosopher's task is in this case to stress the need for theories explaining what has been described.

Take for example a reaction of the form (23) that happens not to occur under normal pressure and temperature conditions unless a catalyzer is present. In the old times catalyzers, such as enzymes, where believed to do their job by just being present. We now explain the effect of a catalyzer in producing a reaction of type (23) by assuming that it occurs in two steps, each of which involves the catalyzer E, which is recovered at the end of the overall reaction. Indeed, we split (23) into two reactions:

$$A + E \rightarrow AE \tag{24a}$$

$$AE + B \rightarrow C + E. \tag{24b}$$

AE is an unstable complex (or rather a species of unstable complexes each composed of one unit of kind A and one of kind E). The above pair is called the *reaction mechanism*, which is supposed to explain the overall reaction. However, this only pushes the problem one step further. We will not have a full explanation as long as we do not know *why* the above intermediate reactions occur – i.e. why the complex AE is formed in the first place and why it combines with B to produce C. No doubt, postulating the intermediate reactions is justified by isolating the catalyst E and regulating the reaction velocities by varying its amount, as well as by isolating and analyzing the complex AE (provided it is not too short-lived). But the intermediate reactions themselves remain to be explained. Physical chemistry supplies this explanation at the macrolevel, particularly with the help of the concepts of affinity and dissociation energy. And quantum chemistry will one day be called upon to explain them at the deeper level of atoms and molecules, of which more below.

Another problem of growing interest to philosophy as well as chemistry is this: How universal a science is chemistry? Or, put in ontological terms: How widespread are chemosystems in the universe? So far chemistry has been mainly Earth-bound, and the few dozen chemical compounds found in other planets, in meteorites, and in interplanetary space, occur on our planet as well. However, the known chemosystems may prove to be only

a smallish subset of the set of all existing chemosystems. Indeed the physical and chemical conditions on the surface of the explored planets are quite exceptional, in that the gravitational, radiation and temperature conditions are midway between those prevailing in the interior of stars and those prevailing in outer space. It is conceivable that at very high pressures, radiation densities, and temperatures, totally different chemical reactions and molecules occur. And it is possible that very short-lived molecules be formed in the interior of stars, in defiance of high temperatures but aided by intense gravitation. Of course this is speculative cosmochemistry. However, we should take such speculation into account before we adopt a geocentric view about the parochialism of chemistry.

To complete the chart of chemical compounds and reactions we need not only more experiments and more space exploration: we also need more theoretical chemistry for, after all, theory alone can tell us which compounds are possible and which ones are not. This platitude warrants repetition because, although chemists do not normally proceed by trial and error like alchemists, but rather are guided by hunches based on well-tried law statements, they do sometimes mistrust theory. Three theories are particularly promising at this time. One is still classical chemical kinetics, which is simple in that it involves only rate equations. However, these equations are non-linear and, moreover, they are systems of equations in the case of simultaneous reactions, like those occurring in most chemical reactors and in all living cells. In addition to such (very severe) computational difficulties, there is the methodological problem of measuring exactly the reaction velocities, which sometimes are huge. If only for its practical complications, good old chemical kinetics will always keep chemists occupied.

A second promising field of theoretical chemistry, and actually a more exciting one, is the merger of classical chemical kinetics with the physical theory of diffusion. This fusion, initiated by the mathematician Alan Turing in 1932, is of particular philosophical importance because it is a clue to morphogenesis, which, though usually described, is seldom explained. Indeed, the coupling of the two processes, chemical reaction and diffusion, gives rise to new patterns, such as propagating or standing chemical waves. Conceivably, such study of morphogenesis at the chemical level will inspire biologists interested in explaining biological morphogenesis. (Not that the process is likely to be the same, but that an explanation of it may come from a merger of previously separate disciplines.)

A third and even more exciting and promising field of theoretical research is physical chemistry. The goal of this study is to explain as much chemistry

as can be explained with the help of physical theories such as thermodynamics, statistical mechanics, and quantum mechanics. An early triumph of quantum chemistry was the explanation of pair bonding with the help of Pauli's exclusion principle. (According to this postulate, any given state of a system of quantons of the same kind, e.g. electrons, can be occupied by at most two quantons. This principle explained the "empirical" generalization that the typical stable molecule contains an even number of electrons – a generalization that led G.N. Lewis to invent the concept of a pair bond as early as 1916) Other triumphs of quantum chemistry have been the explanation (at least in principle) of molecular spectra and the qualitative prediction of the existence of certain compounds. True, the number of exact results is small, and even the approximate results concern only a tiny fraction of all the kinds of molecule known to exist. However, the introduction of quantum mechanics into chemistry has effected a conceptual revolution. Even if they cannot calculate everything they would like to, chemists now think in a radically new way – e.g. in terms of potential barriers, ground and excited states, molecular orbitals, and scattering cross sections.

Quantum chemistry is expected to succeed where classical chemistry, in particular the classical electronic theory of valence, had failed – such as the existence of covalent bonds (as in H_2), of molecular spectral bands, and of individual chemical reactions (as in the collision of two atoms resulting in the formation of a molecule). But so far, around 99% of quantum chemistry has concentrated on the properties of light molecules: it is quantum chemical statics. Only a few papers deal with quantum chemical dynamics – the theory of chemical reactions –, which ought to become the heart of theoretical chemistry. Fortunately this field has gained vigor since about 1970, particularly since the method of crossed molecular beams was employed to study individual chemical reactions. (This is the direct method of altering the atomic composition of molecules. Classical chemistry knew only the indirect method involving myriads of atoms. Note the parallel with molecular genetics and classical genetics.)

The existence of quantum chemistry poses in new terms the old problem of reduction: Is chemistry merely a branch of physics? To be sure chemistry borrows many concepts from physics: witness those of atom, temperature, and specific heat. But it also mints concepts of its own, such as those of chemical reaction and catalyzer, affinity and reaction velocity. However, no sooner is a chemical concept proclaimed to be peculiar to chemistry, than someone attempts to reduce it to physical concepts. An early case was that

of chemical purity, which pre-atomic chemistry was incapable of defining. (It gave *criteria* of chemical purity, such as the constancy of melting and freezing points, but no *definition* proper. The contemporary definition is: "Substance x is chemically pure = $_{df}x$ is composed exclusively of either atoms or molecules of a single species or kind".) A more recent example is the concept of chemical structure, which has been held to be alien to physics (Woolley 1978), but proved to be reducible to (definable in terms of) physical concepts after all (García-Sucre and Bunge 1981). Therefore nearly every quantum chemist harbors the secret hope that every single chemical concept will eventually prove to be defined in terms of physical concepts.

Yet the hard fact is that the successes of quantum chemistry have been qualitative rather than quantitative. One reason is admittedly that it involves tremendous computational difficulties, e.g. handling determinants with tens of thousands of entries. Yet it may also be the case that only the simplest problems in chemistry can be tackled *ab initio*, i.e. by posing and solving the Schrödinger equation. It may well be the case that medium and large molecules have emergent properties – such as a definite shape and the self-duplication of the DNA molecule – that are classical rather than quantal. In short, it may well be that many molecules – possibly all the 10^{12} or so kinds of protein, and the 10^{10} or so species of DNA – are semiquantons rather than quantons. If this is the case, *ab initio* calculations are foolhardy, and a semi-classical theory will have to be invented. This would be required not just because of the computational difficulties of purely quantum-mechanical models of molecules and reactions, but because medium and large molecules have emergent properties representable by concepts alien to quantum theory.

The radical reductionist is likely to rejoin that the problem of reduction is artificial for, after all, things and their properties do not come labeled 'physical' or 'chemical': they are said to be physical, or chemical, only because they happen to be conceptualized by physicists or chemists respectively – a division of labor that is conventional and subject to historical changes. Granted. However, it must also be owned that chemosystems and their molecular products have properties of their own (Vol. 4, Ch. 2), and that physics, though necessary to explain chemistry, is not enough. For one thing, there is the matter of the objective emergence of new properties, i.e. of properties possessed by systems but not by all of their components (Vol. 4, Ch. 1). For example, a body of water is not fully characterized by saying that its molecules are composed of two hydrogen atoms and one oxygen atom. The fluidity, transparency, low viscosity, low conductivity,

and large dielectric power of water are macroproperties rooted to its composition but not understandable in terms of it alone. Witness the large differences between steam, liquid water, snow crystals, and ice cubes – not to speak of droplets and streams. For another, quantum mechanics does not seem capable of explaining molecular evolution. Not that this process is mysterious, but its explanation calls for not only a knowledge of atoms and molecules but also some knowledge of their interactions with a complex and variable environment that cannot be adequately represented by a Schrödinger equation because it is a process occurring at the interface between quantons and classons.

However, the problem of the reducibility of chemistry to physics deserves a new section, not only because of its intrinsic interest but also because its investigation may be used as a model for investigating similar problems, such as that of the reducibility of biology to chemistry, or of psychology to biology. In what follows we shall make heavy use of a previous study (Bunge 1982b).

7.2. *Is Chemistry Reducible to Physics?*

Opinion is divided as to whether chemistry is an independent science or a branch of physics. Experimental and classical theoretical chemists are likely to defend the autonomy of their science, pointing out that it has its own peculiar concepts, such as those of chemical bond and chemical reaction; its own peculiar law statements, such as reaction and rate equations; and its own peculiar experimental techniques, such as titration and chromatography – not to speak of its own professional societies and journals. On the other hand quantum theoretical chemists are likely to defend the opposite thesis, reminding us that even the old atomic theory explained the classical laws of chemical combination as well as the meaning of the stoichiometric formulas (such as "CO_2"); that the theory of molecules is but an application of quantum mechanics to systems composed of nuclei and electrons; and that the theory of chemical reactions is in principle an application of the quantum theory of scattering to the inelastic collision of atoms and molecules. If asked why, this being so, quantum chemistry has so far solved so very few problems, they may reply that it is all just a matter of computation ability: that "in principle", or "conceptually", the problem has been solved.

Who is right, the autonomist or the reductionist? To answer this question we need to compare, not just the referents of the theories of chemistry to those of physics, but all the aspects of the two fields of inquiry, We shall do so with the help of our characterization of a field $\mathscr{R}$ of scientific research

(Ch. 1, Sect. 1.1). This allows us to introduce a definition that will be found useful not only in this section but also in subsequent chapters:

DEFINITION 2.1 If $\mathscr{R}_i = \langle C_i, S_i, D_i, G_i, F_i, B_i, P_i, K_i, A_i, M_i \rangle$, for $i = 1,2$, denote two fields of scientific research at a certain time, then $\mathscr{R}_2$ is *included* in $\mathscr{R}_1$ (or $\mathscr{R}_2$ is a branch or chapter of $\mathscr{R}_1$) if, and only if (i) C_2 and S_2 are subsystems of the community C_1 and the society S_1 respectively, and (ii) every coordinate (component) of $\mathscr{R}_2$ beyond the second is included in the corresponding coordinate (component) of $\mathscr{R}_1$. (The inclusion relation is $\subseteq$.)

Let us check whether the ten relations occurring in the above definition hold among the components of physics ($\mathscr{R}_1$) and chemistry ($\mathscr{R}_2$). To begin with, it is empirically false that the chemistry community C_2 is a subsystem of the physics community C_1, even though some individuals work both as physicists and as chemists. Indeed each of those systems has its own professional and scientific organizations, and its own information network. On the other had, usually the society hosting C_1 and C_2 is one and the same – i.e. $S_1 = S_2$, the trivial case of the subsystem relation.

As for the domains, at first sight that of chemistry (D_2) is included in that of physics (D_1) because chemical systems would seem to constitute a special kind of physical systems. But this impression is mistaken, for what are physical about a chemical system are mainly its components, as the system possesses certain emergent chemical properties in addition to some resultant physical properties. Indeed, recall that a chemical system is a system wherein chemical reactions occur, i.e. one the atomic and molecular composition of which is variable. Since system and components do not share all their properties, they do not belong to the same species of thing. In short, it is not true that $D_2 \subset D_1$; rather, the composition of a chemosystem is included in D_1

On the other hand, the philosophical background G_2 of chemistry is properly included in that of physics. In particular, for most purposes chemists adopt an atomistic or corpuscularistic ontology: field-theoretic ideas enter only through quantum theory. As for the formal backgrounds F_1 and F_2, they may be taken to be identical. It is true that chemistry makes little use of certain mathematical tools, such as differential geometry, that field physicists find indispensable. However, this restriction may be temporary: it is advisable to have the totality of mathematics on call.

Divergences reappear from the sixth coordinate on. To begin with it is not true that the specific background B_2 is included in B_1. Rather, chemistry draws liberally from physics, i.e. $B_2 \subset K_1$. But, since physics employs a

modicum of chemical knowledge, we also have $B_1 \cap B_2 \neq \emptyset$. (For example, solid state physicists need to know the chemical composition of the substances they study.) In sum, it is not true that $B_2 \subset B_1$ and $K_2 \subset K_1$. As for the problematics of the two sciences, they overlap partially; but, since chemistry studies a domain of facts that is not included in the domain of physics, it is not the case that P_2 is included in P_1. The general aims of chemistry are the same as those of physics, namely description, explanation, and prediction; however, since chemistry studies facts that are not covered by physics (i.e. $D_2 \not\subset D_1$), the specific aims of chemistry are not included in those of physics: $A_2 \not\subset A_1$. Finally, for this very reason $M_1 \not\subset M_2$. In sum, we have proved that *chemistry is not included in physics*.

However, there is consensus that the study of chemosystems *depends* upon a knowledge of physical entities: one also says that the former is *based* on the latter. What is not always clear is the very concept of epistemological dependence of one field of inquiry upon another. Fortunately all the elements for an elucidation of this notion are at hand. A first obvious condition for the dependence relation between two fields of inquiry to hold is that they have something in common, for otherwise neither could help the other. A second condition is that the specific background B_2 of the dependent field be included in the fund of knowledge K_1 of the other. A third condition is that the very formulation of every problem in the dependent field require the solution to some problems in the other. We assume that these three conditions are necessary and sufficient for the dependence relation to hold. And we lay down the following formal definition for ease of reference:

DEFINITION 2.2 The research field $\mathscr{R}_1 = \langle C_1, S_1, D_1, G_1, F_1, B_1, P_1, K_1, A_1, M_1 \rangle$ *precedes epistemologically* (or is presupposed by, or serves as a *basis* for) the research field $\mathscr{R}_2 = \langle C_2, S_2, D_2, G_2, F_2, B_2, P_2, K_2, A_2, M_2 \rangle$ at a certain time if and only if, at that time,

(i) $\mathscr{R}_1$ and $\mathscr{R}_2$ *intersect*, i.e. at least one of the coordinates of $\mathscr{R}_1$ beyond the second has a non-empty overlap with the corresponding coordinate of $\mathscr{R}_2$;

(ii) the specific background B_2 of $\mathscr{R}_2$ is *included* in the fund of knowledge K_1 of $\mathscr{R}_1$;

(iii) *every* problem in P_2 is implied by *some* problem(s) in P_1 – i.e. formulating and investigating any problems in P_2 requires the previous solution to some problem(s) in P_1.

It is easy to see that physics precedes chemistry in this sense, just as mathematics precedes physics, and chemistry precedes biology. Indeed, condition (i) is satisfied because $G_2 \subset G_1$ and $F_2 \subseteq F_1$; condition (ii) is satisfied as well because chemists make use of, *inter alia*, thermodynamics and quantum mechanics; and condition (iii) too is satisfied because the very formulation of every chemical problem presupposes having solved certain physical problems, such as measuring weights or calculating entropies. So, *chemistry is based on physics* in the sense of Definition 2.2.

We shall support our conclusion by examining the relation between quantum chemistry and quantum physics, the more so since doing quantum chemistry looks very much like doing quantum physics – so much so that the logo of the Quantum Chemistry Group (Uppsala, Sweden and Gainesville, Florida) is Ψ. We shall do so by using the concepts of concept reduction and theory reduction. To start with the concepts of quantum chemistry: whereas some of them (e.g. those of atom and state function) are borrowed from the quantum theory of atoms, others are not. We say that a chemical concept B is *fully reducible* to physics if it is definable exclusively in terms of a (simple or complex) physical concept A, i.e. if $B =_{df} A$. The concepts of valence and molecular structure, though typically chemical, are definable in terms of the quantum theory of atoms. On the other hand the very concept of molecule is not so definable. Indeed a molecule is a whole described by a state function the properties of which are determined not only by its physical components but also by their interaction. So much so, that a molecular state function is not built by combining the state functions of the constituent atoms, but is found by solving the Schrödinger equation for the molecule. Likewise the concept of rate constant, central in the theory of chemical reactions, is not fully reducible to physics even though the value of the rate constant(s) of any chemical reaction can in principle be calculated with basic quantum mechanics plus certain subsidiary assumptions.

(The concept of rate constant is defined in *classical* chemical kinetics by reference to the phenomenological rate equations. The quantum-theoretical calculations cannot even start without explicit reference to the corresponding classical rate equation(s). For example, the rate of formation $\dot{n}_C$ of the reaction product C in a bimolecular reaction of the form "$A + B \rightarrow C$" is given classically by $\dot{n}_C = kn_An_B$, where n_X is the concentration of chemical of kind X. An aim of quantum chemistry is to *calculate* k, defined classically, in terms of the probability of collisions of A's with B's, instead of leaving it as an unexplained empirical parameter. See Polanyi and Schreiber (1973) for the theory, and Bunge (1982b) for a detailed methodological analysis.)

The concepts of molecule and of rate constant are cases of partial concept reduction. This methodological concept can be defined as follows. Let A be a physical concept, B a chemical one, and C a chemical proposition not deducible from physical propositions alone, such that "If C, then $B = A$". We call this a *partial reduction* of B to physics.

As for *theory* reduction, we shall use the definitions introduced in Vol. 6, Ch. 10, Sect. 3.1. Let $\mathscr{T}_1$ and $\mathscr{T}_2$ be two theories the domains or reference classes of which have a nonempty intersection. Further, call Δ a nonempty set of reductive definitions (or full concept reductions), and A a nonempty set of hypotheses not included in either $\mathscr{T}_1$ or $\mathscr{T}_2$ but couched in the language of $\mathscr{T}_1$. Then (i) $\mathscr{T}_2$ is *fully* (or *strongly*) *reducible* to $\mathscr{T}_1$ iff the union of $\mathscr{T}_1$ and Δ entails $\mathscr{T}_2$; and (ii) $\mathscr{T}_2$ is *partially* (or *weakly*) *reducible* to $\mathscr{T}_1$ iff $\mathscr{T}_2$ follows logically from the union of $\mathscr{T}_1$, Δ, and A. Moreover, we say that $\mathscr{T}_1$ *helps explain* $\mathscr{T}_2$ iff $\mathscr{T}_1$ is mechanismic and $\mathscr{T}_2$ phenomenological, and $\mathscr{T}_2$ is fully or partially reducible to $\mathscr{T}_1$.

Our discussion of the concept of rate constant suggests that quantum chemistry explains classical chemistry, but that it is only *weakly reducible* to the quantum theory of atoms. We shall sketch a proof of the latter assertion. The question is to ascertain whether the set A of subsidiary assumptions occurring in the preceding definitions is empty, i.e. whether or not quantum chemistry follows from quantum mechanics without further ado. It is easy to pinpoint members of A; only, some of them look so obvious after half a century that theorists hardly bother to state them explicitly, and therefore fall victims to the illusion that quantum mechanics entails chemistry. Among such subsidiary assumptions we count the following:

A_1 Every molecule is composed of nuclei and electrons. [Note the difference with the classical atomic theory, according to which atoms are the units of chemical analysis.]

A_2 All the interactions among the components of a molecule are electromagnetic [even in the case of homopolar bonds].

A_3 Every molecule has an equilibrium state which is that of lowest energy (i.e. its ground state).

A_4 Every chemical reaction is either elementary or the outcome of a number of simultaneous elementary processes of the same kind.

A_5 Every chemical reaction consists in the combination, dissociation, or substitution of atoms or polyatomic systems, such as molecules and radicals. [This hypothesis is borrowed from classical chemistry.]

In addition to such extra substantive hypotheses one also introduces a number of methodological ones. The strongest among these are simplifi-

cations such as neglecting the motion of the nuclei, or assuming this or that electron to be far away from the nuclei, or even neglecting whole components of the molecule, such as the sugar-phosphate backbone of DNA. Moreover, most quantum chemical calculations involve guessing zero approximation state functions. Even more to the point: many calculations in quantum chemistry, particularly in chemical dynamics, involve classical statistical mechanics and even classical billiard ball models (Levine and Bernstein 1974, Miller *et al.* 1976). In such cases the quantum-chemical models are semiclassical. Only for comparatively simple molecules and chemical reactions can we speak of the partial or weak reduction of quantum chemistry to the quantum theory of atoms. (See also Lévy 1979, Primas 1983.)

To sum up, (i) whereas some chemical concepts are definable in terms of physical concepts, others are not; (ii) the quantum theory, by itself, fails to entail quantum chemistry: the latter follows only when enriching the former with extra assumptions, so that quantum chemistry is based on quantum physics but is not part of it; (iii) chemistry, regarded as a field of inquiry, has a number of traits which it does not share with physics – hence chemistry is not a branch of physics; (iv) nor is chemistry an autonomous science, for it is based on physics. However, in turn chemistry feeds physics a number of data, ideas, and problems. So, chemistry and physics are *interdependent*. The explicit recognition of this interdependence has always benefited both sciences. On the other hand isolationism, or purism, has caused stagnation or at least missed opportunities.

8. MEGAPHYSICS

8.1. *Earth Science*

In this section we shall glance at the physics and chemistry of planets, in particular our own. That is of course earth science, which comprises geology, geochemistry, geophysics, oceanography, and meteorology. So far, earth science has been the object of only a few philosophical studies, mostly by practitioners of the science (e.g. Albritton 1963, Kitts 1974, 1977, Riccardi 1977). However, it poses a rich and interesting philosophical problematics, as shown by the following sample. Is planetary science exclusively observational or also experimental? Is it only descriptive or also explanatory and predictive? Does the fact that it is a historical science alienate it from physics and chemistry? Does it have concepts, hypotheses

and methods of its own, or are they all reducible to physical or chemical ones? And what has been the impact of the plate tectonics revolution, initiated in the 1960s, on the methodics of earth science? Let us examine a few such problems.

There are two rather popular misconceptions of the nature of earth science (Kitts 1974). One is that it is an exclusively *observational* discipline not an experimental one. Although this description does apply to most of the work in the field, it fails to cover all of it. In fact, geologists and seismologists make experiments on models, and meteorologists experiment with miniclouds in ice boxes, as well as with natural clouds (e.g. by seeding them with crystals). The difference between earth science and the other factual sciences, with respect to experiment, is one of degree and phase of development, not one of kind (Riccardi, 1977).

A related popular belief is that earth science is an exclusively *descriptive and inductive* discipline, one that proceeds from case to case and from sets of similar cases to timid empirical generalizations. This belief too is false: earth science is not any more inductive than other sciences. Far from restricting himself to descrining what he sees, the earth scientist frames or borrows hypotheses to explain his data – in particular, hypotheses about the unobserved evolution of the planet. And, since most of the hypotheses he borrows from physics belong to hypothetico-deductive systems, he often engages in deductions.

Another received view is that, because earth science is undoubtedly a *historical* discipline, it must be radically different from physics and chemistry (Simpson 1963). This opinion is only partly true. Firstly, earth science has its synchronic aspect as well. Secondly, only the land masses have long term memory: the atmosphere and the oceans (unlike the ocean floors) have only short term memory, so it is impossible to trace their history except indirectly, via the land masses. So, earth science is only *partly* historical – just like astronomy. Thirdly, it is not the case that physics and chemistry are totally non-historical. In fact, physics studies materials with structural or magnetic memory: recall metal fatigue and the common magnetic tape. And the study of molecular evolution, so important to biology, is a growing chapter of chemistry. Ergo, physics and chemistry too are historical, though only peripherally so for the time being. But even if the historical portions of physics and chemistry were excised, earth science would not be alienated from physics and chemistry. If it were it would be impossible to use physical and chemical laws to account for the evolution of the planet. In short, earth science is *both* historical and firmly based on physics and chemistry (Kitts 1974).

That earth science makes constant use of physical and chemical law statements, is well known. For this reason it satisfies one of the conditions for being ranked as a science (Vol. 6, Ch. 14, Sect. 2.1). Besides, earth science has a number of generalizations expressing regularities in the evolution of the planet – e.g. Steno's stratigraphic generalizations such as the "law of superposition". But, since these generalizations do not belong to full-fledged hypothetico-deductive systems, they do not qualify as law statements in our rather restrictive sense (Vol. 5, Ch. 9, Sect. 3.2). In sum, at least for the present earth science has no laws of its own: it borrows laws from physics and chemistry. Thus seismology is an application of elasticity theory; meteorology, one of hydrodynamics and thermodynamics; and geochemistry, of chemistry.

Two reasons may be adduced for the absence of laws peculiar to earth science. One is that it is a comparatively young science. However, this won't do because there are even younger sciences, such as psychology, that boast of a few laws. A deeper reason may be that there *are* no objective and universal historical patterns of the evolution of planets – or of anything else for that matter. After all, a history is not a law but the *result* of laws and circumstances (e.g. initial composition, boundary, and constraints). In other words, a law is a bundle of possible trajectories in some state space, rather than a single trajectory (Vol. 5, Ch. 9, Sect. 3.2). This being so, the very search for historical laws, in earth science or elsewhere, is "mistaken in principle" (Simpson 1963 p. 29). All we can hope to find is definite *trends* in a given history.

At first sight earth science has not even concepts of its own: it would seem that it contains only specializations of physical and chemical concepts, as well as peculiar names to designate them. Thus geological strata and tectonic plates form special kinds of bodies, seismic waves are a particular case of elastic waves, and 'Earth' is just a name of a particular planet, planets being only a kind of mechanical object. True, but such specializations are *unique* to earth science: they do not occur in basic physics or chemistry, and they make sense only in earth science. Thus the concepts of rock and sand, of mountain and river, of karst and volcano, of aurora borealis and earthquake, of wind and rain, are characterizable only with the help of a comparatively large number of physical or chemical concepts each. In sum, earth science *does* have concepts of its own.

The recent plate tectonics revolution has not altered the nature of geology. For one thing, it has been as respectable a scientific revolution as any in that, far from cutting all links with the past, it has grown out of the previous

period, it retains many of its achievements, and it keeps physics and chemistry as constraints not to be violated. Thus the plate tectonics revolution does not quite exemplify Kuhn's original catastrophic view (Kitts, 1977). Rather, it illustrates that blend of evolution and revolution we all call 'evolution' (Vol. 6, Ch. 13, Sect. 3.2).

The most dramatic effect of the plate tectonics revolution has been to catalyze the clumping of the branches of earth science that evolved nearly in parallel. (See e.g. Smith Ed. 1981.) In turn, this synthesis has enhanced considerably the explanatory and predictive powers of earth science. In fact geology, which had focused attention on gradual processes, such as sedimentation and erosion, is now capable of explaining at least some of the catastrophes occurring in the terrestrial crust. The explanatory power of the theory can be gathered from what I take to be its five postulates: (i) The Earth's crust is composed of a few plates (at latest count 15 shells about 100 km thick each). (ii) The plates are torsionally rigid and they float on the mantle. (iii) The plates fit rather closely together. (iv) The plates are in slow motion, sometimes rubbing against each other and at other times creeping underneath their neighbors (subduction). (v) The frictions at the plate boundaries generate seismic waves that propagate throughout the Earth's crust.

Much remains to be known about the plates, in the first place the kind of forces that drive them. However, even in its present young stage, plate tectonics theory has contributed powerfully to explaining the present configuration of mountain ranges and ocean bases, as well as many earthquakes. It also explains continental drift, which had been laughed out of academia when first proposed by Wegener in 1919. (Wegener's failure to propose a mechanism for the drift, along with the dogmatism with which gradualism was adhered to, was the undoing of his hypothesis – until it was resurrected.) In turn, continental drift explains such orographic processes as the formation of the Himalayas as resulting from the collision of India against China. It also solves such biogeographical puzzles as certain commonalities in the flora and fauna of Africa and South America, which had once been united. And it may also explain certain remarkably quick speciations and extinctions in the history of life on our planet. In short, the hypothesis of continental drift explains a large mass of previously disconnected and unexplained data. (The latter constitute indirect or circumstantial evidence for the hypothesis, which so far has not been confirmed by direct measurement because of the slowness of continental drift, estimated to be on the order of a few centimeters per year.)

Plate tectonic theory focuses on catastrophes, but these are not the only events in the history of the planet. There are also gradual processes, such as sedimentation and erosion, that constitute the continuous background occasionally interrupted by quick violent processes. In this regard earth science is no different from any other historical science: both gradual and catastrophic changes have their legitimate places. Hegelians might say that this is a dialectical synthesis between the traditional catastrophism of the "successive creations" and gradualism. Nothing of the sort, for ancient catastrophism had been miraculous rather than scientific, and because the catastrophes envisaged by contemporary earth scientists (as well as by evolutionary biologists) are really processes – only, far quicker than the others.

Plate tectonics theory has renewed some disciplines, such as paleogeography, the task of which is to reconstruct the distribution of land masses in the past – and perhaps even to forecast their future distribution. In turn, paleogeography has shed intense light on evolutionary biology, and it is likely to alter some ideas in paleoanthropology. Last, but not least, that revolution has led to the emergence of a *new system* of knowledge out of a number of previously rather disconnected disciplines. The new system is, of course, earth science.

In conclusion, there is no question that earth science, though firmly based on physics and chemistry, is a distinct science. In fact it is not only cultivated by a distinct scientific community, but it has concepts of its own (e.g. that of geological stratum), problems of its own (e.g. describing and explaining changes in landscapes), methods of its own (e.g. surveying with the help of Landsat satellites), and aims of its own (e.g. reconstructing the history of our planet and locating mineral resources). All it needs, to catch up with the rest of the physico-chemical sciences, is more imagination of the kind that led to formulating the plate tectonics theory (and its precursor, Wegener's continental drift hypothesis) and the design of the Mohole project. A realistic philosophy of earth science should contribute to stimulating that imagination.

8.2. *Cosmology*

Cosmology was born thousands of years ago in close association with religion and philosophy. (See e.g. Munitz 1957.) It acquired something resembling scientific status in the 19th century, when it was first conceived as the mechanics and thermodynamics of the universe. Since then it has undergone two revolutions. The first was brought about in the 1920s by the

application of Einstein's theory of gravitation to the universe as a whole. The second revolution started about 1970 and is still in progress: it resulted from an uneasy merger of Einsteinian cosmology with "particle" physics – whence the name "quantum cosmology".

Each of the three transformations fired the popular and literary imaginations, but only the first made a deep impression on philosophers. Yet contemporary cosmology poses a large number of interesting philosophical problems, such as: Is cosmology possible given that it deals with a single thing and that there is no science but of the general? Are there cosmic laws? Could the universe have been created, and if so was it designed as the home of man? If not, what evidence could there possibly be obtained about the stage preceding the "big bang"? And Is the universe bound to end up in a state of homogeneous disorder? (See also Kanitscheider 1979, Harrison 1981.)

Let us start with the first question. It has often been claimed that cosmology cannot be scientific because it concerns a single entity, whereas laws, which are essential to science, are universal. This view rests on too narrow a conception of natural laws. To be a law statement a formula need not be referentially universal: it suffices that it be spatially or temporally universal. For example, the schema "For all times thing b satisfies condition L" is admissible as a law schema. With this proviso it is clear that the formulas of cosmology, such as the Robertson-Walker line element, will become law statements the day they are empirically confirmed. When this happens they will not differ in general structure from the laws of motion of the Moon, or of Mars, which are unique to those bodies. On the other hand there is room to doubt that the Hubble "law" ($v = H_0 r$), which relates the velocity of a moving light source to its red (or blue) shift, is a law proper. It may be merely an empirical generalization expressing a trend in the observed part of the universe: it may prove to be false in regions yet to be explored or in future epochs. After all, at the time of writing the data are consistent not only with the hypothesis of monotonical expansion but also with that of an eventual contraction.

Another crucial problem still to be solved is that of the spatial finitude or, alternatively, infinity, of the cosmos. The metric tensor (or line element) shared by most contemporary cosmological models, *viz.* Robertson–Walker's, contains a constant k that must be determined empirically on the strength of measurements of the average matter density in the universe. If $k > 0$, the universe is closed (spatially finite), whereas if $k \leq 0$, the universe is open or flat (spatially infinite). Current data are so imprecise that they are

compatible with the two alternatives. Until this crucial point is resolved, the said metric cannot be called a *law*: it is a speculative conjecture.

However, even if cosmology had no laws of its own, i.e. even if all of its hypotheses were speculative, it would meet our definition of a science (Vol. 6, Ch. 14, Sect. 2.1) insofar as it uses laws borrowed from other fields – in particular the laws of gravitation and of "particle" physics. In this regard cosmology does not differ from its humbler yet more prosperous neighbor, earth science. Indeed both are *predominantly historical sciences based on physics* (as well as on chemistry in the case of earth science).

Granted, cosmology is still largely speculative. But speculation can be sound or wild, scientific or unscientific, according to whether it harmonizes, or fails to harmonize, with well-founded general hypotheses, i.e. law statements (Bunge 1983e). Hence any failure of a particular cosmological model to agree with the physics of the day can be used to indict it as unscientific. For example, steady state cosmology – the only serious rival of the big bang model between 1950 and 1965 – became unscientific the day it incorporated the *ad hoc* hypothesis of the continuous creation of matter out of nothing (Bunge, 1962b).

Nineteenth century cosmology yielded plenty of food for philosophical thought. The Kant-Laplace hypothesis on the origin and evolution of nebulae (our galaxies) became a pillar of the naturalistic and evolutionary world view. On the other hand the Newtonian model of the universe suggested the idea of eternal recurrence (actually invented in Antiquity). If the universe is a huge (perhaps infinitely large) gas in a steady state, then any given event is likely to recur any number of times. In particular – perish the thought – this very page may have been written before, and may be written again, by a perfect replica of the present author. This boring model was soon replaced by a pessimistic one deriving from an application of the second law of thermodynamics to the universe as a whole. It was suggested that the universe is running downhill: that its total energy, though quantitatively conserved, is becoming degraded. It was forecast that the universe will eventually end up in a state of maximum entropy or "thermal death", where no macrophysical novelty could ever happen.

All three models were eventually modified or even discarded. The Kant-Laplace hypothesis is too simple to be true: it ignores quantons. (Yet, it must be admitted that we have no good alternative theory of the origin of galaxies, much less one compatible with the fashionable big bang model.) On the other hand the Newtonian model of the universe as a whole, as a sphere of uniform density, has been rendered compatible with the expansion

hypothesis and it yields a remarkably good approximation. As for the thermodynamic version of eschatology, it has been given up because the second principle holds only for closed systems, and the universe is not enclosed in a container. (Remember Sect. 2.2.) Besides, astronomers have discovered plenty of anentropic (or build-up) processes, such as the birth of new stars and galaxies, along with break-down processes.

Since about 1920 nearly all cosmological models have been compatible with Einstein's theory of gravitation. The most popular of them are the *evolutionary* or Friedmann models, which involve the Robertson–Walker line element mentioned a while ago, and assume that the constant k occurring in it is non-negative (i.e. that space is closed). The best known of all these models is the "standard hot big bang model", according to which (*a*) initially the universe was concentrated in a comparatively small "fireball" at about 10^{12} degrees Kelvin; (*b*) the ball exploded for some unknown cause about 2.10^{10} years ago, and the universe has been expanding ever since; (*c*) at the present time matter and radiation are distributed homogeneously throughout the universe.

Hypothesis (*a*) implies that the universe is spatially finite and, together with hypothesis (*b*), that its present radius is about 2×10^{10} light years. This is sometimes interpreted in theological terms, as proving that the universe was *created* out of nothing. Thus Jastrow (1978 p. 12): "the astronomical evidence proves that the Universe was created twenty billion years ago in a fiery explosion". This statement contains three misleading words: 'evidence', 'proves', and 'created'. Firstly, as we all know, no empirical evidence can prove definitely a hypothesis – particularly if the latter is out of step with the naturalistic world view inherent in modern science. Secondly, the available evidence supports also alternative models; more on this point below. Thirdly, the word 'created' is out of place in cosmology: all the standard hot big bang model does assume is that the universe *started to expand* about 2×10^{10} years ago. Moreover the model assumes explicitly that the universe existed prior to that time, though in an entirely different state, namely as a "fireball". Hence it is a gross distortion to hold that "the astronomical evidence leads to a biblical view of the origin of the world" (Jastrow 1978 p. 14). Astronomy does not assume that the universe was "born", let alone "created", when the fireball exploded. If it did it would be unscientific, for science abides by the principles that nothing comes out of nothing or turns into nothingness (Vol. 3, Ch. 1, Sect. 1.3), and that everything happens according to law rather than miracle (Vol. 3, Ch. 4, Sect. 2.3). In short, it is not true that contemporary cosmology supports creationism,

and that science is at one with theology, as Jaki (1974), Jastrow (1976), Davies (1983) and others have claimed. Natural theology cannot be revived at this late date.

The great methodological question concerning the standard hot big bang model is the quality and quantity of the data in support of it – particularly since only the favorable evidence is usually cited. This evidence consists of the following items: (*a*) the model includes the best theory of gravitation we have, namely Einstein's: (*b*) the model predicted correctly the existence of the 3 °K background radiation, coming and going in all directions, and supposed to be a relic of the primeval explosion (as well as a universal reference frame); (*c*) the model explains the redshift of the spectra of nearly all distant galaxies. Point (*a*) is important but not crucial, because many alternative models are compatible with the same theory of gravitation. Point (*b*), the most astonishing prediction of the model, is its war horse. I submit, on the other hand, that point (*c*) is its Achilles's heel, for alternative explanations of the redshift are conceivable – and because it is shared by all expanding models. Let us pause for a moment and glance at the redshift controversy.

The redshift is a well known fact: what is at issue is its explanation. Theorists analyze the redshift into three components; (*a*) Doppler effect (due to the mutual recession of the galaxies), (*b*) gravitational (due to the action of gravitational fields on light), and (*c*) cosmological, or due to the expansion of the universe (wavelengths stretch as space itself expands). By framing special hypotheses and using certain additional observational data, astronomers can often ascertain the relative contributions of these three redshift sources. This analysis is then anything but a matter of routine – so much so that component (*c*), usually estimated by subtracting the sum of components (*a*) and (*b*) from the total observed value, should be handled with great caution.

The upshot of the preceding discussion is this. Since nobody can *see* the galaxies receding (Doppler effect) or space expanding (cosmological redshift), astronomers try to explain ("interpret") the observed redshifts with the help of well known physical laws, speculative hypotheses, and additional data. Neither of the last two are fool-proof. (The sun is the exception: here only the gravitational redshift is important and is known with accuracy. On the other hand the quasars, which exhibit huge redshifts, have been a source of spirited argument ever since they were discovered by the Burbidges: see e.g. Field *et al.*, 1973.)

Given the difficulty of the subject there should be nothing surprising

about this controversy: there is no growing science without controversy. What is anomalous is the fact that most astronomers do not seem to recognize the controversial nature of the matter, but adopt the standard model uncritically and, "knowing that their subject is this area already rests on rather shaky foundations as far as hard-proven evidence is concerned, cannot face up to the opening of Pandora's box in extragalactic astronomy" (Burbidge 1981). In this situation a detailed methodological examination of the redshift controversy would prove useful for an objective evaluation of the existing cosmological models as well as to facilitate the inception of new ideas. However, there will be room here only for a few remarks. (See Tolman 1949 and Bunge 1955c for criticisms of the dogmatism with which the early version of the big bang model was adopted.)

The defenders of the standard hot big bang model assert that it accounts for *all* the known facts. This is not quite true. Firstly, the model assumes that the universe is spatially finite (closed), although the known values of matter density at present favor an infinite (open) model (Liang and Sachs 1980). Secondly, the model treats matter as if it were distributed continuously and homogeneously throughout the universe, and so it collides head-on with the most conspicuous observational feature of the universe, namely its inhomogeneity or local clumping into stars, star clusters, galaxies, etc. (Neyman 1963). Thirdly, the hypotheses about the first few nanoseconds of the expansive phase are brilliant speculations, but it is still to be proved that they are consistent with Einstein's theory of gravitation, which is about classons, not quantons. (It is far from obvious that a matter tensor built with the help of quantum theory, as demanded by "quantum cosmology", results in a meaningful geometric tensor. The union of general relativity and quantum theory should involve the quantization of the gravitational field, and this is still a goal to be achieved.) Fourthly, according to the standard model, space should be rather strongly curved; but astronomical observation suggests that, although space curvature does occur in the vicinity of large bodies, space in the large is very nearly flat – this being why some Newtonian cosmological models are a good approximation. Fifthly, the largest estimated intergalactic distance is about twice the distance (in light years) the age the model attributes to the expansive phase of the universe. These and other difficulties have prompted a number of astronomers to look for alternatives, among which the so-called hierarchical models (involving many levels of clumping) stand out.

In sum, although the standard cosmological model does account for some of the known facts, it is inconsistent with others. And even if it did account

for all the data we should be cautious in accepting it , because the data are neither as numerous nor as accurate as to exclude alternative models. After all, many a cosmological hypothesis has proved to be no better than myth clad in beautiful mathematics (Alfvén 1977). The myth of the creation of the universe is one of them. A related myth is the "anthropic hypothesis", according to which the universe has been designed as the home for man (Carter 1974). According to this wild speculation the various fundamental physical constants would have values and relations that, far from being contingent, would point to the emergence of man at a suitable place and time. This exercise in neo-Pythagoreanism gives only the order of magnitude of the said constants and it lacks predictive power (Carr and Rees 1979). Moreover the logic is wrong, for from the fact that man emerged at the place and time he did, and not somewhere else at a different time, all that follows is that it was physically possible, not necessary, for our species to appear there and then.

To conclude. Cosmology is currently thriving as never before but, at the same time, it risks entering a barren phase for the premature adoption of one of the various models compatible with both fundamental physical theory and astronomical data. It is in dire need of a strong dose of critical realism.

9. Concluding remarks

Physics and chemistry pass for being the "hardest" of all sciences. Yet some of their areas – particularly "particle" physics, quantum chemistry, and cosmology – are far less developed than certain areas of biology, such as genetics, or even social science, such as history. Even the most successful physical theories contain grave unsolved difficulties, such as the infinities and virtual entities of quantum electrodynamics. Nor is there any doubt that the foundations of physics and chemistry are remarkably immature, partly because so few scientists work on them. But there is an additional cause of this backwardness, namely the operationist philosophy still adhered to by most physicists and chemists although it was refuted long ago by philosophers. Therefore philosophers have a chance and a moral obligation to meddle with the foundations of physics and chemistry – provided of course they are reasonably well acquainted with them.

We have only sampled the problematics of the philosophy of physics and chemistry. The following short and haphazard list of open problems should

persuade anyone that the field needs many more laborers. What kind of science is meteorology: basic or applied, homogeneous or mongrel? Does the quantum theory of solids exemplify strong (full) or weak (partial) reduction? What is the root of the "arrow of time"? Is it true that quantum theory has rendered ontology impossible by welding the object to the cognitive subject? Are genuine elementary building blocks of nature likely to exist? Is it reasonable to blame so many unexplained astronomical anomalies on such conjectured entities as superluminal "particles" and black holes? We could go on and on, but we must stop here.

In Part II we shall jump from physics and chemistry to the life sciences. However, we shall meet again, under a different guise, some of the problems encountered in the present chapter, in particular those of anthropomorphism and reduction. But of course we shall also encounter some new problems – which would not be the case if biology were a mere continuation of physics and chemistry.

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